MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Professor H. H. TURNER, D.Sc., F.R.S., VICE-PRESIDENT, in the Chair.

G. A. Hemsalech, 6 Place de la Sorbonne, Paris;
Griffith Parry Jenkins, Hamilton, Ontario, Canada;
Richard Henry Norman, M.D., B.S., Canonbury Park South, N.;
Harry Edwin Wood, M.Sc., Chief Assistant, Government Observatory, Johannesburg, South Africa; and
Julien Péridier, 9 rue Gimelli, Toulon, France,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Alec Joscelyne Bamford, B.A., Assistant Astronomer to the Ceylon Government, The Observatory, Colombo, Ceylon (proposed by F. J. M. Stratton);
Rev. Basil Staunton Batty, Bolsover Vicarage, Chesterfield, and Oxford and Cambridge Club, Pall Mall, S.W. (proposed by Charles Pendlebury);
George Frederick Dodwell, B.A., The Observatory, Adelaide, South Australia (proposed by Sir Charles Todd); and
Rev. William Francis Rigge, S.J., Professor of Physics, and Director of the Observatory, Creighton University, Omaha, Nebraska, U.S.A. (proposed by the Rev. Walter Sidgreaves).

The following were proposed by the Council as Associates of the Society :—

Dr. W. de Sitter, Professor of Astronomy in the University of Leyden; and
Professor Karl Schwarzschild, Director of the Observatory, Göttingen.

Sixty-six presents were announced as having been received since the last meeting, including, amongst others :—

Astrographic Chart; 18 charts, presented by the Royal Observatory, Greenwich; 13 presented by the Tacubaya Observatory, and 41 by the French Minister of Public Instruction, from the Algiers, Bordeaux, and Paris Observatories.

Practical Suggestions on Mathematical Notation and Printing.

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It is a subject of common complaint that mathematical manuscripts are often prepared for press without due regard for the difficulties encountered in setting up the type, or for the appearance of the printed page.

The Council of the Royal Society have had under consideration for some time the desirability of taking steps with a view to diminish the expense of printing and proof-corrections, and to avoid waste of space, and undue variety of notation in papers by different authors in the same volume. They have approved of the reprinting, with modifications and additions, of the substance of a Report to the British Association on this subject, in the hope that greater uniformity and facility in mathematical typography may thereby be promoted. The recommendations which follow are now offered, not in any authoritative way, but simply as a consensus of opinion; to this end it is understood that they were submitted in advance, for consideration and criticism, to the Council of the London Mathematical Society.

*Abstract of Report of British Association Committee.**

With a view to the questions referred to them for consideration, the Committee appointed by the British Association made inquiries into the nature and processes of mathematical printing, and the difficulties attendant thereon; and it appeared to them that a statement of the results of these inquiries would form the best introduction to the suggestions which they had to make.

The process of "composition" of ordinary matter consists in arranging types uniform in height and depth (or "body" as it is termed) in simple straight lines. The complications peculiar to mathematical matter are mainly of two kinds.

* Report of the Committee, consisting of W. Spottiswoode, F.R.S., Prof. Stokes, F.R.S., Prof. Cayley, F.R.S., Prof. Clifford, F.R.S., and J. W. L. Glaisher, F.R.S., appointed to report on Mathematical Notation and Printing, with the view of leading mathematicians to prefer in optional cases such forms as are more easily put into type, and of promoting uniformity of notation.—*B. A. Report*, 1875, pp. 337–339.

First, figures or letters of a smaller size than those to which they are appended have to be set as indices or suffixes; and consequently, except when the expressions are of such frequent occurrence as to make it worth while to have them cast upon type of the various bodies with which they are used, it becomes necessary to fit these smaller types in their proper positions by special methods. This process, which is called "justification," consists in filling up the difference between the bodies of the larger and smaller types with suitable pieces of metal.

The second difficulty arises from the use of lines or "rules" which occur between the numerator and denominator of fractions, and (in one mode of writing) over expressions contained under radical signs. In whatever part of a line such a rule is used, it is necessary to fill up, or compensate, the thickness of it throughout the entire line.

The complications above described may arise in combination or may be repeated more than once in a single expression; and in proportion as the pieces to be "justified" become smaller and more numerous, so do the difficulties of the workman, the time occupied on the work, and the chances of subsequent dislocation of parts augment.

The cost of "composing" mathematical matter may now (1908) in general be estimated at somewhat more than twice that of ordinary or plain matter, the recent adoption of the point system in the casting of types having greatly simplified mathematical justification.

There are many expressions occurring in mathematics which are capable of being written in more than one way; and of these some present much greater difficulties to the printer than others. This being so, the Committee were of opinion that instead of making any specific recommendations, the most useful course they could take would be to append a table of equivalent forms specifying those which do and those which do not involve justification, and also a list of mathematical signs which may fairly be expected to be found, in the usual sizes, ready to hand among a printer's materials.

In recommending in this qualified way some forms of notation in preference to others, the Committee wished it to be distinctly understood that they were speaking from the printing, and not from the scientific point of view; and they were quite aware that, even if some of the easier forms should be adopted in some cases, they may still not be of universal application, and that there may be passages, memoirs, or even whole treatises in which they would be inadmissible.

The Committee drew attention to the advantages which may incidentally accrue to mathematical science by even a partial adoption of the modifications suggested. Anything which tends towards uniformity in notation may be said to tend towards a common language in mathematics; and whatever contributes to cheapening the production of mathematical books must ultimately assist in disseminating a knowledge of the science of which they treat.

Mathematical Signs not involving "Justification."

$$\times \quad - \quad + \quad = \quad \surd \quad \pm \quad :: \quad \therefore \quad \because \quad : \quad \} \quad < \quad > \quad \div$$

$$(\quad [\quad \} \quad \int \quad \surd$$

$$a \quad a' \quad a_1 \quad a^2 \quad a_2 \quad a^{\frac{1}{2}} \quad a_{\frac{1}{2}}$$

Equivalent Forms.

Involving justification.	Not involving justification.
$\frac{x}{a}$	x/a or $x \div a$ or $x : a$
$\sqrt{x}$	$\surd x$ or $x^{\frac{1}{2}}$
$\sqrt[3]{x}$	$\sqrt[3]{x}$ or $x^{\frac{1}{3}}$
$\sqrt{x-y}$	$\surd(x-y)$ or $(x-y)^{\frac{1}{2}}$
$\sqrt{-1}$	ι or i
$x\,.\,\overline{x+a}$	$x(x+a)$
$\epsilon^{\frac{n\pi x}{a}}$	$e^{n\pi x/a}$

This British Association List, which has been abbreviated and modified, is now incorporated in the following :—

Recommendations regarding Mathematical Notation and Printing.

Always—

instead of	$\frac{x}{3}$	$\frac{a+b}{2}$	$\frac{a+\frac{b}{2}}{\frac{c}{3}+\frac{d}{4}}$	$\frac{a}{b+\frac{c}{d}}$	$\sqrt{x}$	$\sqrt{-1}$	$\frac{1}{x}$
write	$\frac{1}{3}x$	$\frac{1}{2}(a+b)$	$\frac{a+\frac{1}{2}b}{\frac{1}{3}c+\frac{1}{4}d}$	$\frac{a}{b+c/d}$	$\surd x$ or $x^{\frac{1}{2}}$	ι or i	x^{-1}
instead of	$\frac{1}{x^n}$	$x\,.\,\overline{x+a}$		$\sqrt{x-y}$	$\epsilon^{\frac{n\pi x}{a}}$	$\int_0^{\frac{\pi}{2}}$	$\lfloor\underline{n}$
write	x^{-n}	$x(x+a)$		$\surd(x-y)$ or $(x-y)^{\frac{1}{2}}$	$e^{n\pi x/a}$	$\int_0^{\frac{1}{2}\pi}$	$n!$

In current ordinary text—

instead of	$\frac{x}{a}$	$\frac{a+b}{c+d}$	$\frac{x}{y+\frac{t}{2}}$	$x/y+\frac{a}{b+c}$
write	x/a	$(a+b)/(c+d)$	$x/(y+\frac{1}{2}t)$	$\frac{x}{y}+\frac{a}{b+c}$

Excessive use of the slanting line, or solidus, is, however, undesirable; it may often be avoided by placing several short fractions or formulæ, with the intervening words if any, on the same line, instead of setting out each one on a line by itself. The last of the examples given above illustrates an improper use, in which symmetry is spoiled while nothing is gained; either both fractions should be written with the solidus, as $x/y+a/(b+c)$, or else neither as above.

The solidus should be of the same thickness as the horizontal line which it replaces; in some founts of type it is too thick and prominent.

Irregularities in the spacing of letters and symbols in the formulæ as printed are often the cause of a general unsatisfactory appearance of the page.

For centimetres, millimetres, kilometres, grammes, kilogrammes, the abbreviations should be cm., mm., km., gm., kgm. (not cms., etc.), and so in similar cases. Present custom is against the use of the signs .˙. and ˙.˙ .

Symbols which are not provided in the usual founts of type are, as a rule, to be avoided. Compounded symbols such as $\dot{a}$ or $\bar{a}$ usually involve justification, and are thus liable to become deranged or broken. The two examples here given have, however, become so essential that separate founts should be provided for them.

The use of a smaller fount for numerical fractions is now customary; thus always $\frac{1}{3}a$ instead of $a/3$. The use of negative exponents often avoids a complex fractional form; as also the use of the fractional exponents, such as $\frac{1}{2}$ and $\frac{1}{3}$. In the latter case $x^{\frac{1}{3}}$ is usually preferred to $x^{1/2}$, notwithstanding that the latter is more legible.

Much is often gained in compactness and clearness by setting out two or more short formulæ on one line, instead of on consecutive lines; in that case they should be separated by spaces, indicated by the sign ♯ on the MS. This would apply with even greater force to expressions such as $x=a,\ =b,\ =c$.

In the Preface to his *Mathematical and Physical Papers*, vol. i., 1880, the late Sir George Stokes successfully introduced the limited use of the solidus notation, obtaining the assent and support of Lord Kelvin, Prof. Clerk Maxwell, Lord Rayleigh, the Editors of the *Annalen der Physik*, and many other mathematicians. He defined its use as restricted to the symbols immediately on the two sides of it, unless a brace or stop intervenes; thus sin $n\pi x/a$ is to mean sin $(n\pi x/a)$; but sin $n\theta./r^n$, in case it is used, would mean $(\sin n\theta)/r^n$.

The yielding of the Earth to disturbing forces. By A. E. H. Love, D.Sc., F.R.S., Sedleian Professor.

[*Abstract of a paper in the Proceedings of the Royal Society, A., vol. lxxxii.*, 1909, *pp.* 73-88.]

The Earth, not being an absolutely rigid body, must be deformed by the tide-generating forces of the Sun and Moon. The object of the first part of the paper is to determine the amount by which the surface actually yields to these forces, or, what is the same thing, the height of the corporeal tide raised by them on the Earth.

It is assumed that, for a first approximation, the Earth may be regarded as spherical, and the surfaces of equal density within it as spheres concentric with the boundary. It is assumed that, for a second approximation, the boundary may be regarded as an oblate ellipsoid of revolution, and the surfaces of equal density also as oblate ellipsoids of revolution, the ellipticities of all these surfaces being subject to the law of "fluid equilibrium." This is the assumption customarily made in the theory of the Figure of the Earth; it does not involve the further assumption of any special law of density, *e.g.* that of Laplace. It is assumed also that, when small disturbing forces, such as the tide-generating forces, act upon the Earth, the deformation produced in it obeys a linear law of elasticity. With these limitations the substance may be compressible or incompressible, it may be possessed of a finite degree of rigidity or be altogether devoid of that quality, and there is an infinite number of ways in which its mass may be distributed.

The effects produced by tide-generating forces in such a body are a deformation of the surface and a state of strain within the body, but, whatever the constitution of the body may be, subject to the limitations stated above, this state of strain and superficial deformation are connected with the forces in a certain way. Let W_2 denote the potential of the tide-generating forces, expressed as a spherical solid harmonic of the second degree. Then the potential of the disturbed body at any point differs from that of the undisturbed body by the addition of a term of the form $K(r)W_2$, where $K(r)$ denotes some function of the distance r of the point from the centre. The value of $K(r)$ at the surface is a certain constant number k, to be determined, if possible, by observation. The deformation of the surface of the body, or the height of the corporeal tide raised on it, is expressible in the form kW_2/g, where g denotes the value of gravity at the surface, and h is a certain number, to be determined, if possible, by observation. When h is known, the amount by which the surface yields to tide-generating forces is known.

The ordinary uncorrected equilibrium theory of the oceanic tides on a rigid body of the same size and mass as the Earth makes the height of the tide equal to W_2/g. Since the potential of the tide-

generating forces near the surface of a yielding body is increased by the deformation in the ratio $1+k:1$, the corresponding expression for the deformation of the surface of an ocean resting on the yielding body, and executing tidal motions according to the equilibrium law, would be $(1+k)W_2/g$. But since the surface of the body is deformed by an amount hW_2/g, the observable height of oceanic tide, or rise and fall of the ocean relative to the body, is $(1+k-h)W_2/g$. In the case of the Earth this expression needs to be corrected for the depth of the ocean, for its self-attraction, and for the existence of continents, and it should then give correctly the height of the tides of long period, such as the fortnightly tide. Apart from such corrections, the heights of these tides are less than the corresponding "true equilibrium heights" in the ratio $1+k-h:1$.

A similar result is obtained in connection with the behaviour of a horizontal pendulum. When sufficient care is taken to eliminate the effects of air-currents, changes of temperature, and the presence of the observer, the observed displacement of the pendulum by the tide-generating force of the Sun and Moon is $1+k-h$ of what it would be if the Earth were absolutely rigid.

The analysis of tidal observations made by Sir G. Darwin for the second edition of Thomson and Tait's *Natural Philosophy*, a more recent reduction of more numerous observations by W. Schweydar,* certain experiments with horizontal pendulums made by Ehlert and v. Rebeur Paschwitz †, and, above all, the recent remarkable investigations of O. Hecker ‡ on the displacement of horizontal pendulums mounted in an underground chamber, concur in leading to the result that the number $1+k-h$ is very nearly $\frac{2}{3}$. One outcome of all this observational work is to establish the approximate equation

$$h-k=\tfrac{1}{3}.$$

The periodic variation of latitude discovered by S. C. Chandler § gives the means of obtaining a second equation by which the numbers h and k may be determined. It has long been known that if the Earth were absolutely rigid, the period of the variations of latitude, if any, would be about 306 days. When Chandler found that the actual period is about 427 days, Newcomb ‖ pointed out that the lengthening of the period must be due to a deformation of the Earth, the figure being adjusted at any time to the rotation about the instantaneous axis. The adjustment of the figure involves a deformation of the surface and an internal state of strain, leading to an inequality in the potential of the Earth; and the amounts of the deformation and the inequality of potential are the same as they would be if the Earth were subject to a

* *Beiträge zur Geophysik*, vol. ix. (1907), p. 41.
† Reduced by Schweydar, *loc. cit.*
‡ *Veröffentlichung d. k. Preusz. geodätischen Institutes*, No. 22 (1907).
§ *Astronomical Journal*, vol. xi. (1891).
‖ *Monthly Notices*, 1892.

certain system of forces. These forces have, in common with the tide-generating forces, the property of being derived from a potential which is a spherical solid harmonic of the second degree, and the effects may therefore be specified by the same two numbers h and k by which the corporeal tides are specified. A theoretical investigation of the period of variations of latitude in the case of a yielding body was made by S. S. Hough,* on the supposition that the body might be treated as homogeneous and absolutely incompressible. G. Herglotz † has shown how to dispense with the assumption of homogeneity. A revision of Herglotz' work shows that it is possible to dispense with the assumption of absolute incompressibility also, and it appears that the period depends upon the number k and not upon the number h.

The equation found for k is

$$k\frac{a\omega^2}{2g} = \left(\epsilon - \frac{a\omega^2}{2g}\right)\left(1 - \frac{\tau_0}{\tau}\right),$$

in which a denotes the mean radius, ω the angular velocity of rotation, g the value of gravity at the surface, ϵ the ellipticity of the meridians, τ the actual period, τ_0 the corresponding period which would exist if the Earth were absolutely rigid. All these quantities are known with sufficient approximation, and it appears that

$$k = \tfrac{4}{15} \text{ nearly.}$$

This result is obtained without using any additional assumption beyond those stated at the beginning of this Abstract, and Larmor ‡ has shown that, of these, the assumption that the ellipticities of the surfaces of equal density are subject to the law of fluid equilibrium may be discarded. The result is therefore established for almost any conceivable constitution of the matter within the Earth.

From the two approximate equations

$$h - k = \tfrac{1}{3} \quad \text{and} \quad k = \tfrac{4}{15}$$

it follows that

$$h = \tfrac{3}{5} \text{ nearly,}$$

or the height of the corporeal tide raised on the Earth is about three-fifths of the "true equilibrium height" of the corresponding oceanic tide.

The remainder of the paper is occupied with discussions of the rigidity which must be assigned to the Earth in order that the equations $h = \frac{3}{5}$ and $k = \frac{4}{15}$ may be verified approximately. For this purpose the substance of the Earth is treated as absolutely incompressible. Lord Kelvin § in his estimate of the rigidity used an equation equivalent to $h - k = \frac{1}{3}$, combining it with the hypotheses of homogeneity and absolute incompressibility, from which

* *Phil. Trans.* (A), vol. clxxxvii. (1896), p. 319.
† *Zeitschr. f. Math. u. Phys.*, vol. lii. (1905), p. 275.
‡ *Proc. R. Soc.*, London, vol. lxxxii. (1909), p. 89.
§ Thomson and Tait, *Nat. Phil.*, part ii., §§ 735–737 and 838–847.

hypotheses it would follow that $k = \frac{3}{5}h$ and $h = \frac{5}{6}$. This method yields rigidity about equal to that of steel ($7{\cdot}6 \times 10^{11}$ dynes per square cm.). If h is taken to be $\frac{3}{5}$ and the substance is assumed to be homogeneous and absolutely incompressible, the rigidity comes out as $1{\cdot}2 \times 10^{12}$ dynes per square cm. If k is taken to be $\frac{4}{15}$ and the substance is assumed to be homogeneous and absolutely incompressible, the rigidity comes out as $1{\cdot}7 \times 10^{12}$ dynes per square cm. The first method makes h right and k wrong, the second makes k right and h wrong, and it appears to be impossible to make both these numbers have the right values if the substance, assumed absolutely incompressible, is also taken to be homogeneous. The two methods, however, agree in making the rigidity much greater than it was estimated to be by Lord Kelvin.

If the Earth is taken to consist of a homogeneous nucleus of density 8·206 enclosed in a homogeneous shell of density 3·2, the ratio of the radius of the nucleus to the outer radius of the shell being 0·78 (Wiechert's law), and if the nucleus and the shell are assumed to be of the same rigidity, Herglotz found that, to make the Chandler period have the right value, 427 days, the rigidity must be about $11{\cdot}68 \times 10^{11}$ dynes per square cm. Schweydar found that to make the displacement of a horizontal pendulum by tide-generating forces equal to two-thirds of what it would be if the Earth were absolutely rigid, the rigidity must be about $6{\cdot}3 \times 10^{11}$ dynes per square cm. He also found that both conditions can be satisfied if the nucleus has rigidity $2{\cdot}02 \times 10^{12}$ and the shell has rigidity $0{\cdot}9 \times 10^{11}$ dynes per square cm. These results show that condensation of mass towards the centre and increase of rigidity in the central portions of the Earth may compensate for a very considerable defect of rigidity in the superficial portions.

This result, however, cannot be pushed so far as to support the hypothesis that the nucleus is separated from the shell by a layer of matter devoid of rigidity, or that there is such a layer within the shell. To illustrate this statement the problem of tidal distortion of a solid nucleus and solid shell separated by a fluid layer is worked out in the paper, and it is found that, even if the nucleus were absolutely rigid, the shell 1400 km. thick, and the fluid layer thin, the rigidity which the shell would need to have, in order that its surface might yield no more to tidal disturbing forces than the surface of the Earth does, would be about five times as great as the rigidity of steel. With a yielding nucleus, a thicker layer of fluid, or a thinner shell, the requisite rigidity would be still greater. It is concluded that the Earth cannot consist of a fluid body enclosed in a solid crust, or of a solid nucleus separated from a solid enclosing shell by a fluid layer. This conclusion would not negative the possible existence of areas of continental dimensions beneath which there might be molten matter, but it would mean that such areas must be isolated; the molten matter could not form a continuous layer separating the inner portions of the Earth's body from the outer portions.

The Relation of the Earth's Free Precessional Nutation to its Resistance against Tidal Deformation. By Prof. J. Larmor, Sec. R.S.

[*Reprinted by permission from the Proceedings of the Royal Society, A., vol. lxxxii.*, 1909, *pp.* 89–96.]

The modern investigation of the wandering of the Earth's axis of rotation, considered as a physical problem relating to the actual non-rigid Earth, may be said to have been initiated in Lord Kelvin's address to the Physical Section of the British Association in 1876. After referring* to the scrutiny of the recorded observations of change of latitudes, conducted by Peters in 1841 and independently by Maxwell in 1851, in search of the regular Eulerian free period of 306 days which would belong to a rigid Earth, with negative results, he insisted that the irregular motions brought out in these analyses are not merely due to instrumental imperfections, but represent true motions of the Pole, due to displacement of terrestrial material. For example, he estimates that existing shifts of material, of meteorological type, are competent to produce displacements of the axis of rotation ranging from 1/2 to 1/20 of a second of arc. A sudden shift of material on the Earth will not at once affect the axis of rotation, but will start it into motion round the altered axis of inertia, with a period of 306 days if the Earth were rigid, which will go on displacing the Pole until it is damped out by the frictional effects of the tidal motions thus originated. A radius of rotation of 1 second of arc would raise an ocean tide of the same period as the rotation, having as much as 11 cm. of maximum rise and fall. Thus the motion of the Pole is to be considered as continually renewed by meteorological and other displacements, as it is damped off by tidal and elastic friction; it was therefore, perhaps, not to be expected that it would show much periodicity, though the movements were eminently worthy of close investigation. Their nature was examined more closely by Newcomb at Kelvin's request; but not much more had been done regarding their cause when Chandler announced that the records of changes of latitude did actually indicate a period of precession—of 427 days, however, instead of the Eulerian period of 306 days, which, if any, had previously been taken for granted. Soon after, in 1890, observations were organised systematically by the International Geodetic Union on the motion of Prof. Foerster, of Berlin; and already, in 1891, he was able to inform Lord Kelvin that a comparison of European observations with synchronous ones made at Honolulu gave direct proof of his conclusion of 1876 (*supra*), "that irregular movements of the Earth's axis to the extent of half a second may be produced by the temporary changes of sea level due to meteorological causes."†

* Reprint in *Popular Lectures and Addresses*, vol ii., see pp. 262–272.

† "Presidential Address R.S.," Nov. 30, 1892; *Popular Lectures . . . .*, vol. ii. p. 504. Lord Kelvin's investigations up to 1876 are collected in *Math. and Phys. Papers*, vol. iii., especially pp. 312–350.

In the following year the synchronous observations had already indicated periodicity, apparently in about 385 days, considerably less than Chandler's estimate, which, however, longer observation has since confirmed substantially. Lord Kelvin remarks in his next annual address as follows:*—"Newcomb, in a letter which I received from him last December, gave what seems to me undoubtedly the true explanation of this apparent discrepance from dynamical theory, attributing it to elastic yielding of the Earth as a whole. He added a suggestion, especially interesting to myself, that investigation of the periodic variations of latitude may prove to be the best means of determining approximately the rigidity of the Earth. As it is, we have now for the first time what seems to be a quite decisive demonstration of elastic yielding of the Earth as a whole, under the influence of a deforming force, whether of centrifugal force round a varying axis, as in the present case, or of tide-generating influences of the Sun and Moon, with reference to which I first raised the question of elastic yielding of the Earth's material many years ago." But "when we consider how much water falls on Europe and Asia during a month or two of rainy season, and how many weeks or months must pass before it gets to the sea, and where it has been in the interval, and what has become of the air from which it fell, we need not wonder" that the amplitudes of the polar wanderings "should often vary by 5 or 10 metres in the course of a few weeks or months."

It will be recalled that the main object of the original calculations of Lord Kelvin, which assign to the Earth as a whole an effective rigidity of the same order as that of steel, was to combat the view then prevalent which assumed for the Earth a fluid interior. Even a solid shell of very considerable thickness, enclosing a fluid core, was thus ruled out, unless its materials were preternaturally rigid; and it is clear that placing a solid core in the middle of the fluid interior cannot affect this conclusion so long as an equilibrium theory is applicable, *i.e.* so long as the layer of fluid material is not so thin or viscous as to prevent its adjusting itself immediately by flow to the alternating tidal stresses impressed upon it from its solid walls. By passing to the other limit, and thus taking it so thin that the outer shell practically rides on the solid nucleus, but without effective tangential stress-connection, we obtain a hypothesis to which this objection does not apply.

In a brief note in *Monthly Notices R.A.S.* this year (1892), Newcomb showed, by a general estimate, that the effect of elastic yielding is competent to prolong the free period to about the amount required by observation. A formal mathematical discussion on the basis of calculation of the elastic deformation of a homogeneous sphere was given by Mr. S. S. Hough, now H.M. Astronomer at the Cape of Good Hope, in a memoir on "The Rotation of an Elastic Spheroid," in *Phil. Trans.*, 1896.

He concluded that the Chandler free period required an effective rigidity of the whole Earth of the order of that of steel,

* "Presidential Address R.S.," Nov. 30, 1892; *loc. cit.*, p. 525.

agreeing with Lord Kelvin's previous estimates from tidal phenomena; and his result seems to have been substantially confirmed by more recent calculations, giving for the average effective rigidity estimates derived from various possible hypotheses and simplifying assumptions ranging between extreme values 17×10^{11} and $4{\cdot}4 \times 10^{11}$, while Hough's estimate was put at $8{\cdot}98 \times 10^{11}$. This shows an even striking degree of agreement in calculations necessarily vague on account of the unknown constitution of the Earth's interior, especially in so far as observations of the equilibrium tides of long periods, and of the deviation of sea level due to tidal attraction, which is essentially the same thing, lead to results of the same order as those of free precessional rotation.* It, indeed, suggests, as we shall actually recognise, that this internal terrestrial constitution really is not involved in these various phenomena, except in the common feature of determining the surface effects arising from a given tidal or rotational stress.† The key to the matter, from the general point of view, is contained in the remark of Hough that the free precession of the yielding Earth is the same as that of a rigid one of the shape that would result when the bulging arising from the centrifugal force of diurnal rotation is removed. It is not difficult to show, from geometrical considerations regarding momentum,‡ that this result is general, and extends to an Earth of any degree of heterogeneity or plasticity. The argument may be reproduced in analytical form and rather wider scope, from another place§ (with definition of I rewritten), as follows:—

Let ω be the angular velocity of the Earth about the instantaneous axis, ω_1, ω_2, ω_3 its components referred to the principal axes in the configuration that the Earth would have if the motion were steady. The Earth is deformed from this configuration by the inequality of centrifugal force due to the deviation of the instantaneous axis from the principal axis, with which it would coincide if the motion were steady. This deforming force is the resultant of the centrifugal force, directed outwards from the instantaneous axis, and the reversed centrifugal force, directed inwards towards the principal axis in question. A linear law of elasticity applies to the small resultant of these two forces. If the same law applied to the two forces separately, the reversed centrifugal force would change the moments of inertia A, B, C to certain values A′, B′, C′, which might, under simplifying hypotheses, be calculated from the theory of the deformation of an elastic sphere; and the centrifugal force directed outwards from the instantaneous axis would produce a certain change of density at each internal point,

* *Cf.* Prof. A. E. H. Love, *Roy. Soc. Proc.*, A., vol. lxxxii., 1908, p. 73. To this paper I am indebted for information as to the results of recent calculations. See also *M.N.*, *supra*, p. 476.

† The identity of these two types runs through the discussions in Thomson and Tait's *Natural Philosophy*.

‡ *Proc. Camb. Phil. Soc.*, May 1896, p. 185.

§ E. H. Hills and J. Larmor, "The Irregular Movement of the Earth's Axis of Rotation," *Monthly Notices R.A.S.*, Nov. 1906, p. 24.

and would raise a certain protuberance on the surface, which might be calculated by the same theory. Let I denote the moment of inertia (about the instantaneous axis) of a mass arranged as specified by this change of density and this protuberance. The instantaneous axis is a principal axis of this mass, and therefore the contributions of this mass to the components of moment of momentum are $I\omega_1$, $I\omega_2$, $I\omega_3$. The complete expressions for the components h_1, h_2, h_3 of moment of momentum are therefore

$$h_1 = A'\omega_1 + I\omega_1, \quad h_2 = B'\omega_2 + I\omega_2, \quad h_3 = C'\omega_3 + I\omega_3.$$

The equations of motion referred to the rotating axes are of the well-known vector type,

$$dh_1/dt - h_2\omega_3 + h_3\omega_2 = L.$$

When A and B are equal, the third of them is

$$\frac{d}{dt}(C\omega_3) = N,$$

where C is the effective moment of inertia $C' + I$: when N is null ω_3 is thus constant, say Ω, up to the first order. The other two equations are

$$\frac{d}{dt} \cdot (A' + I)\omega_1 + (C' - B')\Omega\omega = L,$$

$$\frac{d}{dt} \cdot (B' + I)\omega_2 - (C' - A')\Omega\omega_1 = M,$$

which in the case of approximate symmetry involve a free period $2\pi(A' + I)/(C' - A')\Omega$, and similarly in the general case, thus depending only on A', B', C' when I is small.

The result is that the period of the free precession is not $C/(C - A)$ days, as it would be for a rigid Earth, but approximately $C/(C' - A')$, where the denominator is that difference of principal moments of inertia which would remain after the imposition of a bodily forcive having as potential

$$W = -\tfrac{1}{2}\omega^2 r^2 \sin^2\theta = -\tfrac{1}{3}\omega^2 r^2(1 - P_2),$$

namely, that of the centrifugal force reversed, P_2 representing the zonal harmonic $\frac{1}{2}(3\cos^2\theta - 1)$.

The first part of W, the term $-\frac{1}{3}\omega^2 r^2$, corresponds to slight contraction of volume, which is immaterial as regards the desired quantity $C' - A'$. The other part, $\frac{1}{3}\omega^2 r^2 P_2$, will produce an extension, of the same harmonic type as itself, along the polar axis, which will in turn alter the potential of the Earth's attraction at its own surface by $k.\frac{1}{3}\omega^2 r^2 P_2$, where the value of k depends on its

effective resistance to deformation. Moreover the Earth's potential is at distant points, by Laplace's formula,

$$V = \gamma\left(\frac{E}{r} + \frac{A+B+C-3I}{2r^3} + \dots\right),$$

which gives

$$V = \gamma\left(\frac{E}{r} - \frac{C-A}{r^3}P_2 + \dots\right)$$

in the present special case; and if, as in the actual circumstances, further harmonics do not occur to sensible amount, this expression holds right up to the Earth's surface. The free surface, of ellipticity ϵ, is

$$\begin{aligned} r &= a(1 + \epsilon \sin^2\theta) \\ &= a(1 - \tfrac{2}{3}\epsilon P_2), \end{aligned}$$

where $a = a(1 + \frac{2}{3}\epsilon)$. The value of ϵ is determined by the constancy over the ocean surface of the total potential $V - W$, as $-W$ is the potential of the centrifugal force, viz. of

$$\gamma\left\{\frac{E}{a}(1 + \tfrac{2}{3}\epsilon P_2) - \frac{C-A}{a^3}P_2\right\} + \tfrac{1}{3}\omega^2 a^2(1 - P_2);$$

whence, equating to zero the coefficient of P_2,

$$\tfrac{2}{3}ga\left(\epsilon - \frac{\omega^2 a}{2g}\right) - \frac{\gamma}{a^3}(C-A) = 0,$$

thus deriving from data of the distribution of gravity, or of the form of the Earth's surface, the value of $C-A$, which determines the astronomical precession. Again, if taking off the centrifugal force would change $C-A$ to $C'-A'$, it would alter V by $\gamma r^{-3}\{(C-A)-(C'-A')\}P_2$, which must, according to the above specification of k, be equal to $k.\frac{1}{3}\omega^2 r^2 P_2$. Thus

$$\begin{aligned} \frac{C'-A'}{C-A} &= 1 - \frac{\frac{1}{3}k\omega^2 a^5 \gamma^{-1}}{C-A} \\ &= 1 - \frac{k\omega^2 a/2g}{\epsilon - \omega^2 a/2g}. \end{aligned}$$

Hence, if τ is the periodic time of actual free precession and τ_0 is what it would be if the Earth were rigid,

$$1 - \frac{\tau_0}{\tau} = k\frac{\omega^2 a}{2g}\Big/\left(\epsilon - \frac{\omega^2 a}{2g}\right).$$

This is the formula (15) in Prof. Love's paper before referred to; it is there deduced from a hypothesis of concentric spheroidal stratification of the Earth's interior, after the manner of Laplace.

We have found that, like Clairaut's formula for gravity, this relation is independent of any hypothesis as to the Earth's internal structure, except such as is involved in the definition and value of k.

As $\omega_2 a/g$ is 1/289 and τ is found to be 428 days, and τ_0 is 306 days, this relation makes k equal to 4/15.

The values of k corresponding to various moduli of rigidity and compressibility of the Earth considered as a homogeneous globe might perhaps be deduced and tabulated for comparison, from Lord Kelvin's and similar elastic analysis.

The height of the long-period equilibrium tides provides different data; corresponding to an extraneous tide-producing potential W_2 of this type, the absolute rise of the water is $(1+k)\,W_2/g$, from which has to be subtracted hW_2/g for the rise of the solid Earth due to this tide-producing potential, thus leaving a factor $1+k-h$ for the relative tide which alone can be the subject of observations. The reductions of tidal data for the Indian Ocean gave Kelvin and G. H. Darwin the value 2/3 for this factor, which is confirmed by more recent discussions: the observations of Hecker with a horizontal pendulum at the bottom of a well, which obviously determine the same thing, viz. the change of level due to tide-producing potential, concur in a remarkable manner. Thus $h = 3/5$.

These values of k and h, as defined in the last paragraph, would not be independent for a homogeneous incompressible globe: they would, in general, require for their consistency both elasticity of volume and of form. The phenomena of free precession give the value of k, but with reference to compression along the polar axis; those of tidal change of level give the value of $h-k$, or rather its mean value, with reference to compression along axes in the neighbourhood of the equator.* This statement is the purest and simplest expression of the information relating to the solid Earth's resistance to deforming forces that the data of periodic change of latitude and of equilibrium (*i.e.* long-period) tides can supply, prior to any hypothesis regarding the internal distribution and the effective elasticity or plasticity of its materials.

[*Added February 2.*—It has been remarked above, after Lord Kelvin, that a sudden shift of material from one part of the Earth's surface to another would alter the position of the principal axis of inertia round which the free precession of the Earth's axis of rotation takes place, and thus cause a sharp bend in the path of the Pole. If the shift were merely local, such as an earthquake may be expected to produce, the effect would be inappreciable. The connection of sharp curvature in the path of the Pole with seismic disturbance, if it really exists, would thus be indirect, the earthquake being itself started possibly by the slight changes,

* *Cf.* Prof. Love, *Roy. Soc. Proc.*, A., vol. lxxxii., 1908, p. 81, to whom this proposition is substantially due, having been reached by him through analysis appropriate to a centrically stratified body. The quantities h and k, in other notation, enter essentially into the tidal discussions by Kelvin and Darwin in Thomson and Tait's *Nat. Phil.*

meteorological or other, of distribution of surface load, which are indicated by the disturbance of the free precession.

But it is to be noticed that a *submarine* seismic subsidence, if uncompensated by adjacent elevation, or *vice versâ*, would be competent to produce sensible direct disturbance of the path of the Pole; for water would have to flow, in part from distant regions, to fill up the defect of level thus produced. The same would be true for earthquake subsidence near coast lines, if it is compensated by rise of the land. In reply to an inquiry on this subject, Prof. Milne writes as follows:—"When a very large earthquake occurs on land, we find vertical and lateral displacements of, let us say, 20 feet, along lines which may be one or two hundred miles in length. The majority of big earthquakes, however, are sub-oceanic in their origin, along lines parallel to mountain ridges, as, for example, at the bottom of the trough which runs parallel to the Andes. The mass movement appears to result in the deepening of the trough and the rise of the coast line. We have measurements where depth has increased as much as 200 fathoms: see *Brit. Assoc. Seismic Report*, 1897, for a number of these measurements."

An estimate of the effect of such displacements is easily made. Thus, an uncompensated subsidence of the ocean floor, of volume corresponding to a fall of one foot over a thousand miles square, in middle latitudes, would produce* a direct shift in the Pole of rotation amounting to about one-eighth of a second of arc; and at the same time the Pole of the principal axis of inertia, round which the 428-day precession of the axis of rotation takes place, would be displaced in the opposite direction through an angle of the same order of magnitude.

In connection with the possibility of irregularity in the Earth's diurnal rotation due to causes of this kind, similar considerations arise.† A slight subsidence, due to shrinkage around the equator, unless it extended downward a long way toward the Earth's centre, would have negligible direct effect on the moment of inertia and, therefore, on the length of the day; but if it were under sea it would involve transference of water from regions nearer the Earth's axis, in order to make up the deficiency, and if the equatorial regions were all under water, a contraction of 50 cm. in equatorial radius would in this way alter the length of the year by an amount of the order of half a second of time, which would be astronomically of high importance.]

* *Loc. cit.*, *Monthly Notices R.A.S.*, Nov. 1906, p. 26.
† Lord Kelvin, *loc. cit.*, § 38.

Further notes on the Position of the Sun's Axis of Rotation, as deduced from observations by C. H. F. Peters in the years 1860–1870. *Papers of the I.U.S.R. Computing Bureau. No. IV.*

By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. The observations made in 1860–1870 by Professor C. H. F. Peters, at Clinton, N.Y., with a 13-inch refractor, were edited by Professor E. B. Frost, and published in 1907 by the Carnegie Institution. The positions of the spots are given, but not their areas. The spots are identified from day to day by marginal letters. In the discussion which follows, all spots are included which persisted for at least 5 days, as indicated by the marginal letter. The total movement between the extreme days was entered, and the number of days; and these totals and numbers of days were all added up for each calendar month, grouping the spots in latitude as indicated in Table I. This table, therefore, does not correspond in detail to those given in the two previous papers for Greenwich measures (*Monthly Notices*, lxviii. p. 98 and p. 609), since the material is given in a different form, but the results will presently be compared with those previously obtained.

TABLE I.

Daily drift in Latitude in each month, during the period 1860–70.

(The unit is 0°·001.)

Latitude.	−25°	−20°	−15°	−10°	0°	+10°	+15°	+20°	+25°
Jan.	—	−117	− 62	− 36	− 18	−54	−41	+ 9	−29	—
Feb.	− 8	−179	+140	+ 79	+119	− 6	+25	+20	+55	+ 11
Mar.	+125	− 56	+ 75	− 36	+ 49	+37	+43	+59	− 4	− 43
Apr.	− 48	+ 13	+ 29	− 51	+ 42	+12	+18	+71	+54	− 19
May	0	+ 10	− 96	+ 41	+ 35	+59	+28	−60	+59	+ 62
June	+ 33	+ 38	+ 1	+ 46	+ 71	+ 4	− 8	−17	+69	− 20
July	—	+ 14	+ 37	+121	+ 33	− 9	− 7	+14	+62	− 12
Aug.	+ 44	− 20	+ 33	− 17	+ 14	−42	+23	+32	+74	+ 89
Sept.	− 38	+ 86	− 49	+ 7	+ 43	−45	+71	−18	+29	−100
Oct.	+ 66	+ 60	− 6	+ 8	− 21	−38	−39	+41	+93	+111
Nov.	+183	− 75	−128	+ 45	− 18	+56	−76	−29	+13	—
Dec.	—	−125	− 50	− 20	− 84	−40	− 1	+56	−80	—
		Means	− 6	+ 16	+ 22	− 6	+ 3	+15		

2. Excluding the outside groups (for latitudes above 20°) we can now analyse the remaining six columns harmonically, to find an expression of the form

$$A \sin \theta + B \cos \theta + C,$$

where $\theta = 0$ for the middle of January.

The results for A and B are as in Table II., those for C being given at the foot of the columns.

TABLE II.

Values of the first harmonic in units of 0°·001.

Latitude.	−20°	−15°	−10°	0°	+10°	+15°	+20°	Mean.
A . . .	+41	− 3	+41	+27	+20	+ 4		+22
B . . .	− 1	−25	−20	− 2	−15	+17		− 8

3. We must now consider what errors in the position of the Sun's axis are hereby indicated. Suppose that in the middle of any particular month the distance of the central meridian of the Sun's disc from the node where the adopted equator cuts the real equator is θ, and that the inclination of the two equators is α'. Then the separation of the equators on the central meridian is $\alpha' \sin\theta$. In longitude β before the central meridian it is $\alpha' \sin(\theta-\beta)$, and in longitude β after the central meridian it is $\alpha' \sin(\theta+\beta)$, and the apparent drift of a spot between these longitudes is thus

$$\alpha' \{\sin(\theta+\beta) - \sin(\theta-\beta)\} = 2\alpha' \sin\beta \cos\theta.$$

If we group together all such drifts for a whole month, the value of θ will change by $\pi/6$, say from $\theta - \pi/12$ to $\theta + \pi/12$; and the average value of the drift is got by integrating $\cos\theta\, d\theta$ between these limits and dividing by the interval. This is easily seen to introduce the factor

$$R = \frac{12}{\pi} \sin\frac{\pi}{12} = 1 - \cdot 012,$$

which has hitherto been neglected as sufficiently near to unity. It can easily be restored if need be.

4. In previous papers β was definitely taken to be either 65° or 35°, and the results have been sufficiently accordant. Hence, in the present instance, where the material is scantier, different values of β have been grouped together to make the most of the observations. Spots which live on the Sun for 5 days or more have all been grouped together, and the average value of β is the angle turned through by the Sun in about 4 days, *i.e.* about 53°·3. The precise value does not matter very much, since β only enters through the factor $\beta/\sin\beta$, owing to the fact that we have divided by the total number of days when we should have used $2\sin\beta$. The value of this factor at 50° is 1·13, and at 60° is 1·21. Probably 1·15 is sufficiently exact for our present purpose, and the reciprocal is 0·87. Taking the angle of rotation in one day as 13°·3, an error of α' in the adopted position of the axis will thus cause a periodic error in the drift for one day, as measured in the present investigation, of

$$\alpha' \times 0{\cdot}87 \sin 13^\circ{\cdot}3 = \alpha' \times \cdot 20.$$

We thus have to multiply A and B by ·060 to convert them into minutes, and then divide by 0·20 to compare with a, the resultant factor being ·30. Hence the mean A is $+6'·6$, and the mean B $-2'·4$.

5. We can now compare the various results as follows, adding Carrington's as starting-point.

Mean Errors of Adopted Position of Sun's Axis.

		A.	B.
Carrington	1853–1861	0′·0	0′·0
Peters	1860–1870	+ 6·6	−2·4
Greenwich	1874–1885	+ 7·3	−0·6
Greenwich	1886–1901	+ 7·4	−7·8
[Greenwich	1902–1905	+12·4	−1·7]

The results for 1902–1905 have not yet been published in detail, and are quite provisional. The reduction was made in order to see whether they would support the large negative value of B given by the period 1886–1901. They apparently do not: an independent reduction including all spots (instead of only those between + 20° and − 20°) gave a positive value for B, viz. $A = +7'·0$, $B = +7'·2$. The value of B is, as has already been remarked, much more uncertain than that of A, as it depends on the comparison of summer and winter photographs.

6. If we consider that the observations as a whole indicate an error in Carrington's adopted axis represented by

$$A = +7'·0 \qquad B = -3'·6,$$

then the errors of the determinations from different zones may be tabulated as follows:—

Errors of Determinations.

	A.			B.		
	−20° to −10°	−10° to +10°	+10° to +20°	−20° to −10°	−10° to +10°	+10° to +20°
1860–1870	−1′·3	+3′·2	−3′·4	−0′·3	+0′·3	+3′·9
1874–1885	−6·9	+1·0	+6·8	+10·8	+3·3	−5·6
1886–1901	−3·8	−1·2	+6·3	−3·7	−9·9	−5·1

The sums of the squares of the nine residuals for A is 174 and for B is 312, which shows the greater uncertainty in the determination of B.

7. One of the important points presenting itself for consideration before a definitive solution can be undertaken is the question of the discordance between the values of A and B from spots north and south of the equator. Up to the present the material has been discussed in a provisional manner in order to see where any systematic differences lay, so that such groups might be kept separate in the final discussion. The grouping of the observations is a matter of great importance in dealing with sun-spots, for near minimum the material is scanty and confined to special latitudes, so that systematic differences may seriously affect the general mean.

Now there seems to be a curious systematic difference between spots north and south of the equator. Let us take the element A, which is better determined than B. We have for the above three periods—

1860–1870	$A_N - A_S$	$= -2'\cdot1$
1874–1885	$A_N - A_S$	$= +13'\cdot7$
1886–1901	$A_N - A_S$	$= +10'\cdot1$
	Mean	$+7'\cdot2$

The first period is discordant, so that the difference might be an idiosyncrasy of the Greenwich results. To test this point solutions were made from the spots of over 20° in latitude, which were excluded from the above solutions, and thus give an entirely independent check. The results were

	From spots over ±20°.	Previous results (double weight).	Mean.
1860–70	$A_N - A_S = +20'\cdot4$	$-2'\cdot1$	$+5'\cdot4$
1874–85	$A_N - A_S = +17'\cdot5$	$+13'\cdot7$	$+15'\cdot0$
1886–01	$A_N - A_S = +4'\cdot2$	$+10'\cdot1$	$+8'\cdot1$
	Means $+14'\cdot0$	$+7'\cdot2$	$+9'\cdot5$

Hence these spots in high latitudes indicate that the small value of $A_N - A_S$ previously found from the period 1860–1870 is accidental; and that on the whole there is a real discordance between spots N and S of the equator. We must thoroughly investigate this discordance, for until the origin of it is understood it is impossible to assign any definite meaning to the deduced position of the Sun's axis, since we are liable to get a different position according as the spots preponderate in one hemisphere or the other. The next step accordingly will be to go over the material again with this point specially in mind. See also *M.N.*, lxviii. p. 101.

8. Coming now to the constant C, the mean value from the six columns in Table I. between latitudes is $+7'\cdot3$, which means a drift of $0°\cdot0073$ per day, or $+0°\cdot20 = +12'$ per rotation. Thus the tentative hypothesis put forward (in *Mon. Not.*, lxviii. p. 103) of a 26 years oscillation must be given up.

There has apparently been only one change of sign in this drift since Carrington's time, and the figures in the paper quoted should be expanded as follows:—

Mean Drift during a whole Rotation.

Carrington.	Peters.	Greenwich.					
1853–1861.	1860–1870.	1874–9.	1880–5.	1886–9.	1890–3.	1894–7.	1898–01.
+18′	+12′	+12′	+36′	−30′	−32′	−56′	−30′

A special examination must therefore be made of the results near 1885, when the change occurred.

Second Note on the number of Faint Stars with large Proper Motions. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. Last November the results of a comparison of ten pairs of plates in zone $+28°$ taken at intervals of 12 to 15 years were communicated to the Society. Out of 2822 stars examined, 16 showed P.Ms. greater than $15''$ per century, and 6 greater than $20''$ per century. Of the 16 stars, 7 were brighter than 9·5 magnitude; and of the 6 with P.M. $>20''$, 3 were bright.

2. The comparison and examination of 30 plates in zone $+26°$ has now been carried out, and I am again glad to acknowledge the volunteer assistance of Mr. J. H. Worthington, F.R.A.S., and Mr. G. H. Hamilton, in remeasuring and checking the cases of possible proper motion. Table I., which follows, shows the general nature of the results. As a rule, all cases have been specially examined where the differences between the plates exceeded $\pm 1''·2$ in either coordinate, and the figures in column 4 give the number of cases where these differences were confirmed on remeasurement and thus probably indicate a P.M., although no stress is laid on these records unless the P.M. exceeds $15''$ per century.

TABLE I.

Zone $+26°$. *Stars having probable large P.Ms.*

R.A. of Plate Centre.		Total Stars compared.	Interval.	Total P.Ms.	Centennial P.Ms.	
					$15''$–$20''$.	$>20''$.
h	m		y			
0	8	162	12·1	3	3	0
0	24	199	14·0	0	0	0
1	4	183	10·9	0	0	0
1	12	227	10·1	1	0	1
1	52	132	8·8	1	0	1
5	36	349	13·1	6	0	0
6	48	303	11·0	5	2	1
7	12	191	9·9	2	1	1
7	44	159	13·0	4	2	0
8	0	145	14·8	5	1	1
8	16	127	15·0	8	2	2
9	4	98	14·9	4	1	3
9	28	126	14·9	2	1	0
10	8	84	12·0	1	0	1
10	24	83	12·0	1	1	0
11	12	65	14·9	5	2	0
12	0	72	14·9	4	0	0
13	4	54	14·9	3	1	0
13	12	35	14·9	5	0	1

TABLE I.—*Zone* +26°. *Stars having probable large P.Ms.*—continued.

R.A. of Plate Centre. h	m	Total Stars compared.	Interval. y	Total P.Ms.	Centennial P.Ms. 15″–20″.	>20″.
13	20	41	14·9	2	0	0
13	44	56	14·9	5	1	0
13	52	91	14·9	5	3	1
15	36	109	12·1	0	0	0
18	32	649	13·9	11	5	0
19	12	734	14·2	4	0	2
20	24	438	12·2	4	3	0
20	32	504	12·2	1	0	1
20	40	515	9·9	4	2	2
21	44	332	14·2	4	0	2
23	20	247	10·9	1	1	0
Totals		6510	...	101	32	20
From previous paper		2822	...	50	10	6
Grand Total		9332	...	151	42	26

3. The important point is brought out by these new results, that the crop of large P.Ms. is not much greater for a plate in the Milky Way containing 800 stars than it is for one in a region far removed from the Galaxy, having only 50 stars on it. To show this, the plates are arranged in Table II. according to the number of stars compared, and the results of the previous paper have been included in order to lengthen the series.

TABLE II.

Plates in Order of Number of Stars.

No. Stars.	P.Ms. 15″–20″	>20″.	No. Stars.	P.Ms. 15″–20″	>20″.	No. Stars.	P.Ms. 15″–20″	>20″.	No. Stars.	P.Ms. 15″–20″	>20″.
734	0	2	308	0	1	191	1	1	98	1	3
649	5	0	303	2	1	183	0	0	91	3	1
515	2	2	294	0	2	182	0	1	84	0	1
504	0	1	264	3	1	162	3	0	83	1	0
438	3	0	253	1	0	159	2	0	72	0	0
431	1	0	247	1	0	145	1	1	65	2	0
359	1	0	230	1	1	132	0	1	56	1	0
349	0	0	227	0	1	127	2	2	54	1	0
332	0	2	199	0	0	126	1	0	41	0	0
310	2	0	191	1	0	109	0	0	35	0	1
4621	14	7	2516	9	7	1516	10	6	679	9	6

There are 10 plates in each group, and the average number of stars compared falls from 475 to 68; but the number of P.Ms. $>20''$ per century is sensibly steady; and though there is a slight drop for those between $15''$–$20''$, it is nothing like the drop in the number of stars per plate.

4. Hence we may apparently expect to find these stars of large P.M. uniformly scattered over the sky, and not becoming more numerous near and in the Milky Way. Do they belong to our "Solar Cluster"?

5. Incidentally, the small number of large P.Ms. per cent. of the stars examined in the last paper is explained; for the plates were chiefly near the Milky Way, and here the percentage of large P.Ms. is least. Taking the average *per plate*, irrespective of the number of stars on it, we get 1·00 per plate for $15''$–$20''$ and 0·60 over $20''$ in zone $+26°$, as against 1·00 and 0·67 in zone $+28°$.

6. The following is a list of the stars having P.M. greater than $15''$ per century on these plates in zone $+26°$.

TABLE III.

List of large P.Ms. in Zone $+26°$.

R.A. of Plate Centre. h	m	Oxford Number. +26°	Cent. P.M. in Arc. $''$	Δx.	Δy.	Interval. y	Mag.
9	4	25798	49·6	−19	−16	14·9	11·2
13	52	34278	47·0	−23	− 5	14·9	11·0
19	12	...	44·5	+ 3	−21	14·2	11·8
7	12	20428*	39·9	−13	− 3	9·9	8·6
8	0	23419*	38·0	− 1	−19	14·8	6·0
21	44	74272*	37·8	−16	− 8	14·2	9·2
9	4	25973*	36·8	− 7	−17	14·9	6·0
13	12	33210*	31·4	−14	+ 7	14·9	8·0
20	40	68464	30·9	− 5	− 9	9·9	11·0
10	8	28107*	30·5	+ 7	−10	12·0	7·5
20	40	67823	27·6	− 2	− 9	9·9	11·6
19	12	55357*	27·3	+ 5	+12	14·2	8·0
1	12	3131*	27·0	+ 9	+ 1	10·1	9·2
20	32	66463*	25·0	− 8	− 6	12·2	9·5
8	16	23968*	24·0	0	−12	15·0	9·2
9	4	25763*	23·4	−11	− 4	14·9	9·0
8	16	24107	22·0	0	−11	15·0	10·0
21	44	74061*	21·6	+ 9	+ 5	14·2	8·2
6	48	19211	20·6	− 3	− 7	11·0	12·0
1	52	4673	20·5	+ 6	+ 1	8·8	11·2
11	12	30034	19·6	− 4	− 9	14·9	10·2
6	48	19480*	19·5	− 6	+ 4	11·0	6·7

TABLE III.—*List of large P.Ms. in Zone* +26°—continued.

R.A. of Plate centre. h	m	Oxford Number. +26°	Cent. P.M. in Arc. "	Δx.	Δy.	Interval. y	Mag.
0	8	355	19·5	+ 5	+ 6	12·1	11·0
6	48	19206	19·2	+ 1	− 7	11·0	12·0
20	24	66226	19·0	− 3	− 7	12·2	11·0
13	52	34208*	19·0	− 9	+ 4	14·9	8·5
18	32	50932	18·7	− 3	− 8	13·9	11·8
7	44	22642	18·6	+ 4	− 7	13·0	10·9
0	8	259*	18·0	+ 7	− 2	12·1	9·2
18	32	50702	18·0	− 2	− 8	13·9	11·5
7	44	22647	17·9	− 5	− 6	13·0	10·8
0	8	272*	17·8	− 5	− 5	12·1	8·8
8	16	24256	17·8	− 8	− 4	15·0	9·0
13	52	34238*	17·8	+ 4	− 8	14·9	9·1
18	32	51050	17·6	− 8	0	13·9	11·8
10	24	28528*	17·5	+ 7	0	12·0	8·9
18	32	50936	17·2	+ 5	+ 6	13·9	10·0
8	0	23412	17·0	− 3	− 8	14·8	10·1
8	16	24255*	16·4	− 2	− 8	15·0	8·9
23	20	78862*	19·2	+ 4	+ 4	10·9	8·7
13	52	34290*	16·2	+ 7	− 4	14·9	9·5
13	44	34103	16·0	− 8	0	14·9	10·4
18	32	51291	15·9	− 7	+ 2	13·9	10·7
13	4	33127	15·8	− 4	− 6	14·9	10·0
11	12	30038	15·8	− 6	+ 4	14·9	11·0
20	24	66211	15·8	− 6	+ 2	12·2	11·8
9	4	25941	15·6	− 6	− 5	14·9	10·0
7	12	20681*	15·3	− 1	− 5	9·9	9·2
20	24	...	15·3	− 6	+ 2	12·2	11·8
20	40	67632	15·3	− 1	− 5	9·9	11·2
20	40	67901*	15·3	− 1	+ 5	9·9	7·5
9	28	26685*	15·2	+ 7	− 3	14·9	9·4

Two stars were not measured on the plates recorded in the Oxford volume, and have therefore no Oxford number. One is a companion to 66211. The stars marked with an asterisk are in the Cambridge Catalogue, and are bright stars; it will be seen that there are 24 out of 50. The magnitudes for these stars are those of the Cambridge Catalogue. For the other stars the magnitudes are inferred from the formulæ in the printed volume, but are to be taken as provisional only.

7. If we now group these stars according to magnitude, adding those of the previous paper to make the list as long as possible, we get results as follows, N denoting the *total* number as bright as, or brighter than, the reading.

TABLE IV.

Showing large P.Ms. for successive Magnitudes.

Mag.	To 7·9.	8·9.	9·9.	10·9.	12·0.
N over 20″	5	10	15	19	25
N 15″–20″	3	8	15	25	41
N over 15″	8	18	30	41	66
log N over 15″	0·90	1·26	1·48	1·64	1·83
Diff.		0·36	0·22	0·16	0·19

8. As regards the average P.M., the material is scarcely sufficient as yet to warrant careful treatment; but if we take the average Δx and Δy, we get results as in Table V. for the P.Ms. given in this paper.

TABLE V.

Octant.	Average. Δx.	Average. Δy.	No. Stars.
h h			
0– 3	+ 7	+1	4
3– 6	0	−3	5
6– 9	−1·5	−6	22
9–12	−5	−5	12
12–15	−2	−1	19
15–18			
18–21	−2	−1	24
21– 0	+1	+2	5

9. It seems probable that these stars are much nearer to us than others of similar magnitude. Hence, if there is any selective loss of light in space, as suggested in *Mon. Not.*, lxix. p. 61, and as the experiments of M. Tikhoff seem to confirm, these stars should be visually fainter than those which appear photographically of the same magnitude.

10. The comparison of other plates is proceeding.

Summary.

On 40 plates in zones + 26° and + 28°, containing nearly 10,000 stars, there are 68 stars with P.M. exceeding 15″ per century, of which 26 have P.M. exceeding 20″ per century. There are also 151 other stars with P.Ms. which are smaller, but fairly discernible with the rather rough methods here adopted. The large P.Ms. are scattered uniformly in R.A., with no condensation towards the Milky Way, and thus probably indicate stars belonging to our Solar Cluster.

On the Theory of Aberration. By H. C. Plummer, M.A.

1. The appearance of Professor Turner's paper on Aberration, in which he has placed in a clearer light the geometrical implications of the accepted law of aberration, suggests that it may be not inopportune to examine, even superficially, the physical aspects of the same question. It is well known to astronomers that recent determinations of the constant of aberration, executed with unimpeachable skill and care, have yielded values which are greater than can be reconciled with the known values of the solar parallax and the velocity of light. Of the three related constants it remains the most uncertain, and the discrepancy suggests either that the simple theory of aberration is not entirely correct, or that the direct determinations of the constant are affected by some source of systematic error which has hitherto been overlooked. The problem is clearly one of great importance, and a review of the position may be useful if it only serves to define the obscurity. The discrepancy is of course very small, yet it is scarcely to be dismissed as a mere result of the accidental errors of observation.

The question will be regarded here from the general standpoint of the undulatory theory, and this rather because it provides as simple a view as any other than because it is to be considered essential. Those text-books may well seem open to criticism in which it is sought to *explain* aberration by simple considerations based on the corpuscular theory of light. The proper view, it may be thought, is to regard the law of aberration as an inductive result of observation which must be taken into account by the physicist in formulating a theory of light.

A different view is, however, possible, and further consideration tends to lead us in this direction. According to the alternative view, the physical importance of astronomical aberration is chiefly historical, and its value in the construction of hypotheses is much less than we might at first suppose.

2. Let us begin by considering the simplest case—that of maximum aberration *in vacuo*. At the time t let fc (fig. 1) be the position of the axis of collimation of a telescope, being defined by a fixed wire at the focus f and the principal point c, and let acb be a plane at right angles to fc. The telescope moves in a direction at right angles to itself, and the axis arrives at the position FC at the time T. Suppose that S is a star, the light from which arrives in a plane wave w, and is converted by the lens system into a spherical wave s. If the light of the star reaches c at the time t and w is parallel to acb, the light will travel from c to the focus f in free ether. Let U be the velocity of light, V the velocity of the telescope, and f the focal length. The passage of the light from c to f will occupy a time $f/\mathrm{U} = \mathrm{T} - t$. In this time the telescope will have moved a distance $f\mathrm{F} = f\mathrm{V}/\mathrm{U}$. Hence the angular displacement of the observed image will be $f\mathrm{CF} = \tan^{-1}\mathrm{V}/\mathrm{U}$. It is assumed here that the lens produces a flat field. If now a bright micrometer wire be

made to bisect the image at f, both the image and the wire will act as coincident sources of light, and no further aberration will occur in the passage through the eyepiece to the retina of the observer.

In order to arrive at this result, which is in accordance with observation, we have made three assumptions: (1) that the light arrives in a plane wave at c, the principal point corresponding to f; (2) that on arriving there the plane wave is instantaneously converted into a spherical wave of radius f; (3) that the wave travels thence to the focus in undisturbed ether. Separately these assumptions are certainly not true, but together they may approximate to the truth. If the lens can fairly be considered as a thin lens in proportion to the focal length, they will not be

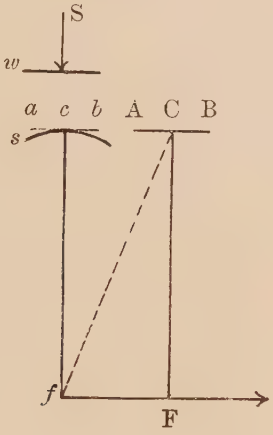


Fig. 1.

greatly in error. But at first sight we have here the possibility of a disturbing factor in the dimensions of the lens system.

For the sake of clearness and simplicity we have considered the case in which the telescope is moving at right angles to the direction of the star. But in any case the motion of the telescope can be resolved into two components, one along and the other perpendicular to the direction of the star. The former merely causes a virtual change in the velocity of light, and to this extent produces possibly a second-order effect. Practically the transverse component will operate as though it alone existed, and the effect to be expected may be inferred from the simpler case already considered.

It must be understood that this is a description of what appears to take place according to the evidence of observation, and not an explanation why it takes place. Otherwise we should encounter the difficulties presented by the much discussed Michelson-Morley experiment, and that is not our intention at present.

3. We turn now to the case of the water telescope investigated experimentally by Airy. Let fig. 2 take the place of fig. 1 when the tube of the telescope is filled with a medium of refractive index μ. If the light disturbance were propagated unaltered along cf it would travel with the velocity U/μ to its focus at f, and the aberrational displacement would be $Ff = \mu f V/U$. The angle fCF is thus the small angle $\tan^{-1} \mu V/U$, but this corresponds to a displacement of the *incident* ray, which may be represented with sufficient accuracy by $\tan^{-1} \mu^2 V/U$. Numerically this corresponds to a maximum aberration of about 36″. Actually, as we know, the Greenwich observers detected no change in the aberration of the star γ Draconis.

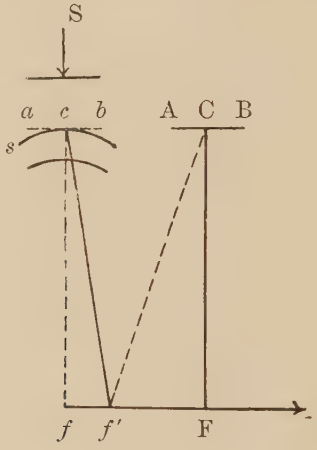


FIG. 2.

If we suppose that this is due to a lateral drift of the waves in the direction of motion, it is easy to calculate the necessary velocity v. The result is that they converge to a focus at f' where $f'F = fV/\mu U$. For this will correspond to the required apparent aberration of the incident ray. It is to be remarked here that the linear displacement $f'F$ is measured by a micrometer which has been calibrated by measuring the known angular distances between external objects, actually by turning the telescope through a right angle about its axis and observing transits of a known star. We also have $ff' = \mu f v/U$. Hence, since

$$fF = ff' + f'F,$$

$$\mu f V/U = \mu f v/U + f V/\mu U$$

or

$$v = V(1 - \mu^{-2}),$$

which is the well-known result, and accords with Fresnel's theory and the celebrated experiment of Fizeau. The physical interpretation can be left on one side.

4. These aspects of the theory are recalled, not because they contain anything new, but that we may have a clear idea of the apparent facts. The result of the experiment with the water telescope seems to show that the astronomical results are unaffected by the fact that they are made in air and not *in vacuo*. Moreover, the interpretation of the experiment leads to the enunciation of a law as to the apparent drift of the light waves in a dense medium, which may be applied to the investigation of a difficulty which we have already noticed. This difficulty is concerned with what happens to the light-disturbance between the time when it reaches the outer surface of the lens as a plane wave and the time when it has just left the inner surface as a converging spherical wave. This point has commonly been left without discussion in astronomical treatments of the question.

The problem is difficult, and it is not proposed to discuss it very deeply here. But in order to draw attention to it, some examination may be given to the simplest possible case, *i.e.* that first discussed above, but with the condition that the path of the light shall be entirely *in vacuo*. This means that the lens system must be dispensed with, and for it must be substituted a parabolic mirror to serve as the image-forming apparatus.

5. Let BAD (fig. 3) be a section of the mirror at the time t when the advancing plane wave has reached the position BD, A

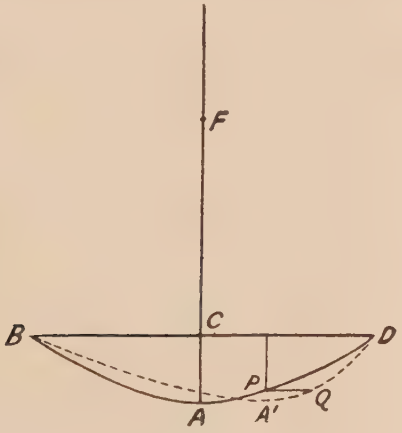


FIG. 3.

the vertex, and F the focus. Take CF as the axis of x and CD as the axis of y, these being fixed in the ether while the mirror is moving in the direction CD.

The equation of the parabola at the time t is

$$y^2 = 4f(x+b)$$

where $CA = b$. The point P of the mirror will not receive an

element of the advancing wave at the time t, but at a subsequent instant when it has advanced to Q. If (x', y') be the point P and (x, y) the point Q we have

$$x = x',\ y - y' = -Vx/U = -x \tan \beta,$$

so that (x, y) lies on the curve

$$(y + x \tan \beta)^2 = 4f\,(x + b),$$

which can also be written in the form

$$(y \cos \beta + x \sin \beta - 2f \sin \beta \cos^2 \beta)^2$$
$$= 4f \cos^3 \beta\ (x \cos \beta - y \sin \beta + b \sec \beta + f \sin^2 \beta \cos \beta).$$

We see at once that the effect of the motion is to cause the figure of the mirror to undergo a virtual deformation. The section considered is still parabolic, but the axis is no longer perpendicular to the circular sections, and the figure ceases to be a surface of revolution. The effect must be to cause a distortion of the image.

The vertex A′ of the deformed section is at the point

$$x = -b + f \sin^2 \beta \cos^2 \beta$$
$$y = b \tan \beta + f \sin \beta \cos \beta\ (1 + \cos^2 \beta),$$

while the focal length is $f \cos^3 \beta$ and the axis is turned backward through the angle β. Thus if F′ is the focus (fig. 4), MN the

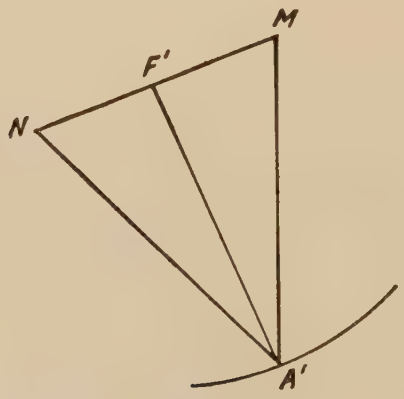


Fig. 4.

trace of the focal plane, and N the point where light coming in the direction MA′ is brought to a focus, $MA'F' = NA'F' = \beta$,

$A'F' = f\cos^3\beta$ and $A'N = f\cos^2\beta$. Hence the coordinates of N are

$$x = -b + f\sin^2\beta\cos^2\beta + f\cos^2\beta\cos 2\beta$$
$$= -b + f\cos^4\beta$$
$$y = b\tan\beta + f\sin\beta\cos\beta\,(1+\cos^2\beta) - f\cos^2\beta\sin 2\beta$$
$$= b\tan\beta + f\sin^3\beta\cos\beta.$$

Now the length of the path from the wave-front BD to N *via* A' is

$$b - f\sin^2\beta\cos^2\beta + f\cos^2\beta = b + f\cos^4\beta,$$

so that by the time the light has reached N the telescope as a whole will have moved through a distance $b\tan\beta + f\sin\beta\cos^3\beta$. Hence to the first order the light-time can be taken as $(b+f)/U$, and the displacement of the image as $f\tan\beta$, which is consistent with the law of aberration. Here we have been considering the light incident on the deformed parabolic section near its vertex. But the path from all points of the circular section BD to F is equal to BF or $b+f$, and hence the light will arrive at F at the same time in the same phase, and will form part of the image. For this part the aberrational displacement will be $(b+f)\tan\beta$. Hence we see that the result of the motion of the telescope is to produce confusion of the image, and that the consequence may be to displace the centre.

6. The actual amount of this virtual distortion of the figure of the mirror is extremely small. The maximum distance between the actual and the virtual surfaces may be expressed with sufficient accuracy by

$$\Delta x = \tan\beta.Dr^2/96\sqrt{3},$$

where D is the diameter of the mirror and r is the ratio of the aperture to the focal length. Now $\tan\beta = 10^{-4}$, $1/96\sqrt{3} = 6.10^{-3}$ and $\lambda = 2.10^{-5}$ in., the wave-length of yellow light. Hence

$$\Delta x = 0{\cdot}03\lambda r^2.D/1 \text{ in.}$$

Thus if r is 1 : 5 and D = 60 in., the diameter of the largest mirror in being, the maximum separation of the real and the virtual surfaces is $0{\cdot}07\lambda$, a quantity just on the verge of what a practical optician will regard as sensible.

7. On the basis of the preceding investigation we may estimate the proportionate effect on the observed aberration as of the order of $+ b/2f$ in the particular case considered. With a ratio of aperture to focal length of 1 : 5, the fraction becomes 1/800, which corresponds to a maximum aberrational effect of $0''{\cdot}025$ additional to the ordinary aberration. The magnitude is small, but it is precisely of the order of the discrepancy which has hitherto remained without explanation.

The question then arises whether this may not be a true explanation, and whether it is not possible that in the more complicated case of a lens the explanation of a residual $0''{\cdot}05$ in

the determination of the aberration constant may be traced to the distortion of the image which arises from the virtual deformation of the surfaces of the lens as a result of the motion through space. In this direction seems to lie a promising clue by which the apparent antagonism between two all-important astronomical constants may be evaded. But it is one which, on further consideration, must be set aside as untenable. The above investigation, it must now be confessed, is based on a faulty idea. We are in fact dealing with one of those seductive errors the close scrutiny of which has been instructive to the writer, and may possibly assist others to clearer views of a singularly elusive problem.

8. Before explaining the answer to the question to which we have been led—very unnecessarily as some may think—it may be profitable to consider another example of the application of our present point of view. Until we can find a more general principle it will be necessary in all questions relating to aberration to consider in detail the path of the light throughout the entire optical system with which we are dealing. A simple and apposite illustration suggests itself readily in the treatment of meridian observations by reflexion. The nadir distance of the reflected ray is equal to the zenith distance of the direct ray if aberration is not considered. In actual practice the nadir distance is treated exactly as if it were a zenith distance, and aberration is applied to both in the same way, as if the two were really the same thing. As a practical rule the method is correct, but the justification for it is not commonly understood. The motion of the Earth may be resolved into three rectangular components, of which we are now concerned with (1) the horizontal component towards the south, (2) the vertical component towards the nadir. The effect of these may be considered successively. Now the effect of (1) is to increase the zenith distance of the direct ray (for a south star), and equally to increase the nadir distance of the reflected ray, so that both observations are affected equally in the same sense. But it is otherwise in the case of (2). The effect of a positive component towards the nadir is to increase the zenith distance of the direct ray, but to diminish the nadir distance of the reflected ray. Hence if both observations are treated alike, we might expect a discrepancy of twice that part of the aberration which arises from the vertical component of the Earth's orbital velocity. What is the explanation of this apparent paradox?

9. It is that we have only considered the purely *apparent* effects of aberration, which depend upon the motion of the telescope. Here there is no real change in the paths of either the direct or the reflected ray. But there is also a real change in the direction of the reflected ray, due to the fact that the reflecting mercury surface is moving parallel to itself. Just as in the case of the concave mirror we were concerned with a virtual deformation of the figure owing to its motion in space, so again here we have a virtual tilt of the plane mirror which is of the same nature.

Let (fig. 5) AB be the position of the incident wave-front at the time when AC is the position of the mirror. The element of the wave at B will reach the mirror at F, where $CF/BF = v/U$, v being the vertical component of the Earth's velocity. Thus the mirror

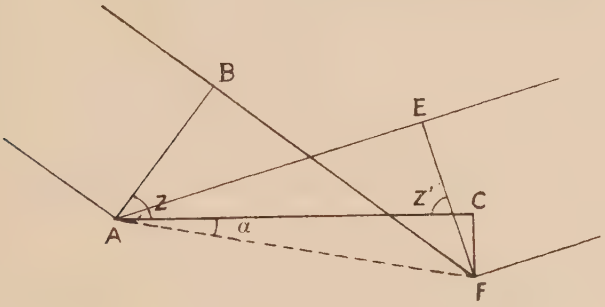


Fig. 5.

is virtually tilted into the position AF, and the light is reflected from the surface AF according to the ordinary law. Hence if FE is the reflected wave-front, AB = FE. Let Z be the angle of incidence BAC, Z′ the angle between EF and AC, and α the angle CAF. Then

$$Z + \alpha = BAF = AFE = Z' - \alpha$$

$$\alpha = \tfrac{1}{2}(Z' - Z)$$

$$\sin \tfrac{1}{2}(Z' - Z) = CF/AF = v.BF/U.AF = v \sin \tfrac{1}{2}(Z + Z')/U.$$

Hence

$$\tan \tfrac{1}{2}Z' = \frac{U + v}{U - v} \tan \tfrac{1}{2}Z$$

which is the law of reflexion for any velocity of the mirror, and to the first order

$$Z' - Z = 2v \sin Z/U.$$

This in the astronomical application is the increase of the nadir distance of the reflected ray owing to the vertical motion of the mercury surface, and it is precisely the quantity required to compensate the discrepancy to which our attention had been led. The above exact law of reflexion at a moving surface is a result given by Professor Hicks* in his discussion of the Michelson-Morley experiment.

10. Now the compensation here is a particular case of Veltmann's theorem,† according to which the common motion of

* *Phil. Mag.*, 1902 Jan. † *Pogg. Ann.*, cl. p. 497.

light source, optical system, and observer can produce no first-order effects in the phenomena of reflexion, refraction, or interference. The general theorem, though simple, cannot be regarded as absolutely obvious. In the meridian observations by reflexion we should find a verification of an exceedingly high order of precision were it not for the existence of the well-known R – D discordance. But the case of the concave mirror considered above seems at first sight inconsistent with the general theorem. The difficulty is, however, easily removed when we realise that it can only be an apparent one.

In the first place the virtual deformation in the shape of the mirror is not the serious matter which we may have supposed without a little closer examination in § 5. Taking a third axis at right angles to the principal section, we have for the equation of the deformed surface on which the wave-front actually falls

$$z^2 + (y + x \tan \beta)^2 = 4f(x + b).$$

This, as stated, is not a figure of revolution. But the equation of the paraboloid of revolution which has the same section in the xy-plane proves to be

$$z^2 \sec^2 \beta + (y + x \tan \beta)^2 = 4f(x + b),$$

which shows that the deviation of the former from a figure of revolution is only of the second order. And now the explanation of the confusion of the image is not far to seek. The incident ray is inclined at an angle β to the axis of the virtual surface of the mirror, and the light is not focussed at a point on the axis. Hence the confusion of the image found above is real enough, but it is due to nothing else than the aberration in the sense in which the word is used in geometrical optics. It is a natural and necessary consequence of the properties of the parabolic mirror, and it has no bearing whatever on the real question we are considering. Properly considered it is precisely the effect which ought to have been expected.

11. A very simple and general proof of Veltmann's theorem has been given by Potier.* On account of its great importance a slightly modified form of proof may be given here. We have seen in § 3 that in a medium moving with a velocity V in the direction of a fixed axis Ox the light-waves drift in the same direction with a velocity $(1 - \mu^{-2})V$. The result is as if the light-disturbance were propagated in the medium at rest while the material points in the medium are moving independently with a velocity $\mu^{-2}V$ in the given direction. Let PQ be an element ds of a path of light, making an angle ϕ with Ox, when the medium is at rest. When it is in motion the corresponding element of path will be PQ′ where QQ′ is parallel to Ox and

$$QQ'/PQ' = \mu^{-2}V/\mu^{-1}U = V/\mu U.$$

* *Journal de Physique*, 1st series, vol. iii. p. 201.

Hence, since

$$\mathrm{PQQ'} = 180^\circ - \phi,$$
$$\sin \mathrm{QPQ'} = \mathrm{V} \sin \phi/\mu\mathrm{U}$$

and therefore

$$ds/\mathrm{PQ'} = \sin \mathrm{PQ'Q}/\sin \mathrm{PQQ'} = \sin (\phi - \mathrm{QPQ'})/\sin \phi$$
$$= \sqrt{(1 - \mathrm{V}^2 \sin^2 \phi/\mu^2\mathrm{U}^2)} - \mathrm{V} \cos \phi/\mu\mathrm{U}.$$

To the first order then

$$ds' = \mathrm{PQ'} = ds + \mathrm{V} \cos \phi . ds/\mu\mathrm{U}$$
$$= ds + \mathrm{V}dx/\mu\mathrm{U}.$$

Now the path PQ′ is travelled with a velocity $\mu^{-1}\mathrm{U}$. Thus

$$\int \mu ds' = \int \mu ds + \mathrm{V}(x_1 - x_0)/\mathrm{U},$$

which shows by Fermat's principle that the path of a ray in the medium at rest, between two points whose abscissæ are x_0 and x_1, must also be the relative path of a ray when the medium is in motion, because if one integral is a minimum the other must be a minimum also; and further, that the retardation in the time of arrival at one point in the medium from another by all rays is constant and equal to $\mathrm{V}(x_1 - x_0)/\mathrm{U}^2$. Hence all possible paths between two given points introduce the same difference of phase, and the motion of the medium gives rise to no first-order optical effect when source and receiver share the same motion.

12. We are now in a position to state the problem of aberration in a clear way, and to distinguish between the astronomical and the physical aspects of the question. The light leaves the source at the time T and arrives at the time t. Let us suppose the source at the time T to be replaced by a fictitious luminous body occupying the same position but moving with the same velocity as the observer. If now we say that the direction of the actually observed ray is the true direction of the fictitious body at the time t, we have three points to consider :—

(1) Whether the apparent direction of the fictitious body really is unaffected by the common motion of itself and the observer through space?

(2) Whether the substitution of the fictitious for the true source of light is legitimate. In other words, is the direction of the received ray independent of the motion of the source in space?

(3) That the direction of the actual source of light can be inferred from that of the fictitious source.

The first two of these points are physical questions. The third is purely a statement of kinematical fact. The three together appear to cover the whole problem, excluding considerations which are foreign, and including what is really germane to the theory.

13. We see at once that by assuming a favourable answer to the physical questions we are left with a simple geometrical problem, the solution of which is effected by the ordinary astronomical rules. The different forms which these rules assume when they are adapted to deal most conveniently with different circumstances have been very fully examined by Professor Turner (p. 403), who has brought out their essential harmony by the detailed discussion of several illuminating examples. From our present point of view the astronomical effect of aberration depends solely on the light-time from the source at time T to the observer at time t.

This means that we are concerned with the actual wave-velocity of light in the medium through which the light travels. If space is filled with a dispersive medium the light-time will be to some extent affected. Recent investigations have suggested that such a medium may exist. But it cannot in any case be competent to exercise any practical influence on observed aberration. For if the light-time were increased in this way by one part in 10,000 the effect on the aberration could not be detected, while, on the other hand, a medium capable of producing an effect of this magnitude would be so strongly absorptive as to render all astronomical observations impossible. The Earth's atmosphere exercises no appreciable effect in this connection, because the light-time through it is merely a very small fraction of a second.

14. Here it may be remarked that the velocity of light does not appear to have been determined by experiment with the extreme certainty which is commonly supposed. By the rotating-wheel method Cornu obtained

300400 km./sec.,

while by the revolving-mirror method Newcomb obtained

299860 km./sec.,

in close agreement with Michelson's earlier result by the same method. The discrepancy, which is of the order of one part in 500, has been discussed very carefully by Cornu,* who concluded that his result was worthy of no less confidence than the other, in spite of its greater probable error. Rosa and Dorsey's recent very accurate determination of the electromagnetic velocity

299710 km./sec.

may be set down here, although it is only made comparable with the other numbers by theory, and it involves a slight uncertainty in the absolute value of the international ohm.

There can be little doubt that the solar parallax is now known certainly by direct determination within one part in 1000. On the

* *Rapports du Congrès de Physique à Paris*, 1900, vol. ii. p. 225.

other hand, the direct determination of the constant of aberration has left the value uncertain by scarcely less than one part in 400. This is unfortunate, and it is of fundamental importance to discover the cause of this uncertainty. Meanwhile the constant of aberration has been determined by the application of the Doppler principle. The detailed account of this work at the Cape Observatory has not yet been distributed, but it is understood that the combination of this result with the accepted value of the solar parallax yields a value for the velocity of light which is in close agreement with Newcomb's experimental result.

15. We may now consider briefly the two physical questions raised in § 12. To the first order, as Veltmann's theorem shows on the simplest principles of wave theory, the apparent direction of a body which shares with the observer a common motion through space is unaffected. Curiously enough, it is this absence of effect which is of the greatest importance to physical theory. But it does not require an astronomical observation for its verification. It is true that the scale of the experiment is immense when it is extended to the furthest stars, and to some minds this may suggest that we are here almost on the verge of a metaphysical question respecting the character of the straight line in space. But in a practical sense the cosmic scale of the apparatus is scarcely relevant. The point can be tested quite as well in the laboratory, and here a far subtler verification can be devised than is to be obtained from the estimated position of a star-image in the field of a telescope, namely, by a well-designed interference experiment. A very wide range of delicate experiments of this kind was executed by Mascart, but the very highest accuracy was reached in the Michelson-Morley experiment, which has been repeated in recent years with increased refinement by Morley. Unfortunately for the theory, not only is the first-order effect compensated, but the expected second-order effect is also absent, although the sensitiveness of the test is adequate to have shown it if it had existed. This null result has led to developments in modern physical theory which are beside our present purpose. The absence of the first-order effect is what directly concerns us. This at least seems certain; and thus it appears impossible that any astronomical observation can be sensibly influenced by the nature of the telescope in relation to its motion through space. The question raised in the early part of this paper is thus answered decidedly in the negative.

The second physical question is concerned with the nature of radiation. We have to admit that the way in which the radiation from a source of light is propagated is quite independent of the motion of the source in space. This is certainly true on the wave theory, and it may be difficult to conceive the contrary. But it appears to be a condition which requires statement.

16. This paper has gone further than was at first intended without throwing any light on the causes of the discrepancies which have been found in the determination of the constant of aberration. But it will have served its purpose if it has rendered

the nature of the problem a little clearer, and if it has helped to close any of those paths which, however inviting, can lead to no solution. In conclusion, the paper may be summarised thus :—

§ 1.	Introductory.
§ 2.	Elementary explanation of aberration *in vacuo*.
§§ 3, 4.	Theory of Airy's water telescope.
§§ 5–7.	Theory of aberration when the image is formed by a mirror.
§§ 8, 9.	Theory of aberration in the case of meridian observations by reflexion.
§ 10.	Reconciliation of the result of §§ 5–7 with Veltmann's theorem.
§ 11.	Proof of Veltmann's theorem.
§ 12.	Distinction between the astronomical and physical principles involved.
§§ 13, 14.	Function and values of the velocity of light.
§ 15.	Consideration of the physical conditions.

Spectroscopic Comparison of o Ceti with Titanium Oxide.

By A. Fowler. (Plate 19.)

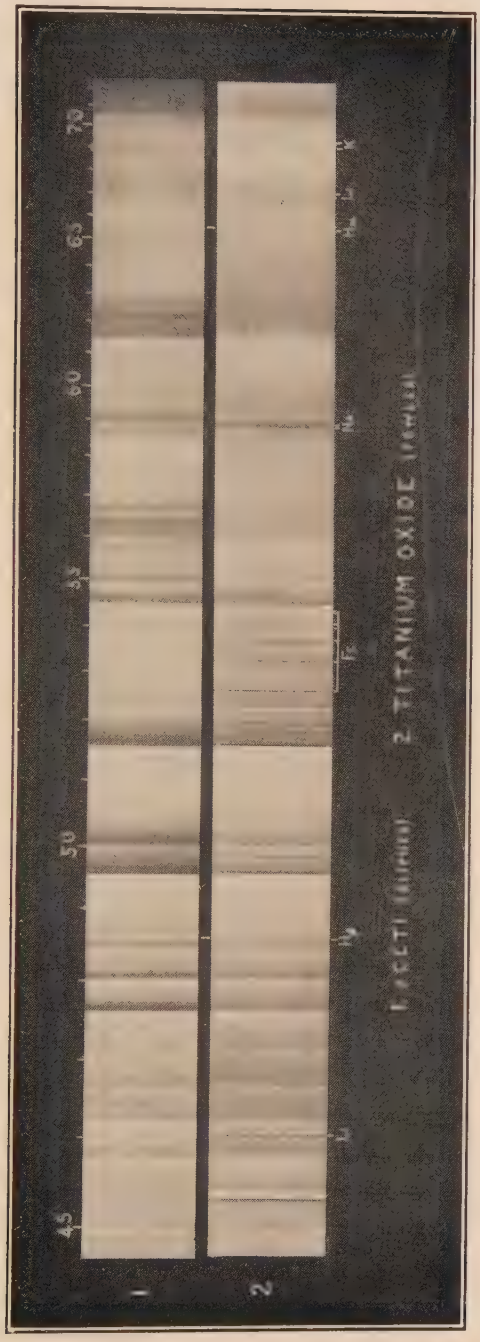
It has already been shown that nearly all of the characteristic flutings of the Antarian or Third Type Stars are produced by the absorption of titanium oxide.* The chief purpose of the present note is to give an additional comparison which strikingly illustrates the truth of the identification.

Having no adequate instruments for the photography of stellar spectra, I appealed to Mr. Slipher, of the Lowell Observatory, for permission to utilise his excellent photograph of the spectrum of *o* Ceti, which is admirably adapted for the purpose in view because it extends from the extreme visible red to the violet.† Mr. Slipher not only consented to this use of the photograph, but very kindly forwarded a special copy for reproduction. The resulting comparison with the fluted spectrum of titanium oxide is shown in Plate 19, in which the stellar spectrum is about five times the scale of the original negative.

The titanium oxide spectrum was photographed on one of Wratten's panchromatic plates with a Littrow spectrograph, giving a dispersion not too great for appropriate comparison with the star. It was obtained from the flame of the electric arc when a considerable quantity of the substance was volatilised between iron poles, and shows a few lines of iron and titanium in addition to the flutings. The D lines of sodium, the red and blue lithium lines at 6708 and 4602, a group of potassium lines about 5800, and the red

* *Proc. Roy. Soc.*, vol. 73, p. 219 (1904). *Monthly Notices* (reprint), App., vol. lxiv. p. 16 (1904). *Proc. Roy. Soc.*, vol. 79, p. 509 (1907).

† *Astrophys. Jour.*, vol. 25, p. 236 (1907).



SPECTROSCOPIC COMPARISON OF o CETI WITH TITANIUM OXIDE.

potassium lines 6939, 6911, appear as impurities. The spectrum is reproduced as a negative in order to correspond with the dark flutings which occur in the spectrum of the star.

Although different instruments were employed in photographing the stellar and terrestrial spectra, the dispersions chanced to be nearly proportional throughout the entire range covered by the flutings. A very satisfactory adjustment of the two was made by giving a slight inclination to the titanium photograph and to the copying-board on which the enlargement was made, according to the method first employed by King.*

It will be seen that the two spectra are for the greater part identical, and that the identification of the flutings is consequently independent of precise determinations of wave-lengths, which are indeed not yet available in the case of the stars. A general indication of the positions of the various flutings, however, will be gathered from the scale marked on the photographs. The wave-lengths of the more conspicuous flutings, as determined from the titanium oxide spectrum,† are also shown in the appended table.

Wave-lengths of Principal Flutings of Titanium Oxide.

	7054·5		5760·15
	7087·8		5811·28
	7125·5		5863·55
	6651·5	Dunér's Band 4	5597·92
	6681·0		5629·53
	6714·1		5661·68
	6748·0		
	6782·0	Dunér's Band 5	5448·48
	6815·1		5496·79
	6850·5	Dunér's Band 7	5167·00
		,, ,, 6	5240·71
	6357·9		
	6384·4	Dunér's Band 8	4954·78
Dunér's Band 1	6416·0		5002·88
	6448·2		
	6479·4	Dunér's Band 9	4761·08
	6512·8		4804·55
	6544·5		4848·20
	6158·86		
Dunér's Band 2	6186·77	Dunér's Band 10	4584·62
	6215·35		4626·49
			4668·82

In each group the more refrangible component is the strongest, except in the case of the third, where 6479·4 is the strongest member of the group.

The new comparison makes an additional contribution to the analysis of the third type spectrum in showing the titanium oxide origin of numerous bands occurring in the red end of the spectrum, the details of which were not recorded by Vogel and Dunér in

* *Science*, 8, 454 (1898). See also Fowler and Eagle, *Astrophys. Journ.*, vol. 28, 284 (1908).

† *Proc. Roy. Soc.*, vol. 73, p. 219.

their visual observations. As stated by Mr. Slipher, the photographic spectrum of *o* Ceti "stops so suddenly at λ7040 as to leave little doubt that another of these bands begins at that point and outruns the sensitiveness of the plate into the red." It is now perfectly clear that this marks the beginning of the group of flutings in the extreme red at λ 7054. The three component bands have in fact been separately identified by Newall and Cookson in α Orionis,* and they are also well seen in a photograph of the spectrum of this star which Mr. Slipher has been good enough to send for inspection. Several narrow bands in the stellar spectrum which fall between 7054 and the strong band beginning at 6159 are also seen to be identical with bands of titanium oxide.

The titanium oxide flutings are evidently reproduced in their entirety in the stellar spectrum, but, as I have previously pointed out, Dunér's band 3, and possibly band 1, is still not completely accounted for. Band 3 begins about 5862, on the more refrangible side of D, and is apparently much darker than the fluting at 5863·5 in the titanium spectrum; Dunér's observations also indicate that in some cases this is one of the strongest bands in the third type stars. Continued search for the possible origin of this band has yielded no definite result. An alternative, and perhaps sufficient, explanation is to suppose that the titanium fluting in this region is made more conspicuous by the inclusion of some metallic lines which remain to be identified.

It now seems quite probable, however, that Dunér's band 1, so far as it is represented in *o* Ceti at least, may be sufficiently explained by the titanium fluting at 6479·4, as there is a close correspondence of the stellar and titanium spectra about this region. The same explanation may apply also to the band in other stars, as the fact that Dunér recorded it in only 22 out of the 297 stars which he so carefully observed suggests that it is comparatively feeble, and only shows itself when the band spectrum is strongly developed, or the star is of more than average brightness.

Besides the bands, the spectrum of *o* Ceti shows bright lines of hydrogen (H_α and H_β appearing in the photograph reproduced), and certain metallic lines. Some of the latter have been shown by Mr. Slipher to be due to vanadium, and the photographs in Plate 19 suggest that others are due to iron and titanium. The D lines of sodium are unexpectedly weak in the star, seeing that they are so strongly marked in α Orionis and other stars of the third type; in reply to my inquiry as to whether they were bright or dark in *o* Ceti, Mr. Slipher states that an examination of the negatives seems to show that they are present as dark lines.

I have pleasure in acknowledging the skilled assistance which has been rendered by Mr. T. Banfield in the preparation of the photographs for reproduction.

* *Monthly Notices*, vol. lxvii. p. 482 (1907).

Note on some Photographs of Jupiter taken near Opposition in 1908 *and* 1909. By J. H. Reynolds.

The photographs on which the following note is based were taken with the 24-in. cœlostat reflector of 38 feet focus, the primary image being enlarged from three to four times with a Cooke negative lens.

The weather conditions have been distinctly adverse to these large-scale photographs during the present opposition, and small detail on the disc has suffered in consequence.

The photographs are valuable, however, in recording the general appearance of the planet, which has been considerably modified since February 1908. Without wishing to unduly depreciate the value of hand drawings, it seems as if the attempt to depict minute detail often results in the general aspect of Jupiter being more or less misrepresented. The scale adopted is sufficiently large to show such phenomena as the shadows of satellites under ordinarily good atmospheric conditions, the exposure required being about 3 seconds for an equivalent focal length of 130 feet. It is by no means certain that comparatively long exposure is altogether a disadvantage in this work, as it has been found that detail becomes more visible as the size of the image is increased, and the exposure consequently lengthened.

This arises probably from the fact that atmospheric disturbance is not confined to bad definition of the planet, but causes the image to oscillate in all directions about its mean position. These movements are sufficiently rapid to leave no trace on the photographic plate if the image is not too bright, and the result is that the mean position is all that is registered. An analogous effect may be observed in long exposures on faint objects such as nebulæ: even if the larger star discs are deformed owing to irregular following, the nebulous details are often sharp and distinct.

An accurate driving clock is of course essential, and electric control is not suitable for large scale work owing to its tendency to produce a "jerky" drive. The driving clock employed with the cœlostat is provided with large governors of the cross-armed type, and this has been found to give satisfactory results.

The plan of work adopted is to make several exposures on the same plate: on average nights about one image out of four is fairly well defined, and sufficiently good for examination and comparison.

The most marked characteristic of the photographs taken this year has been the lack of contrast between the belts and the zones, which contrast was very marked early in 1908.

Visually, the belts now appear to be of a light brick-red colour, the principal exception being the north equatorial belt, which is dark grey. This distinction is well shown on the photographs, in which the position of the north equatorial belt bordering on the equatorial zone is always the darkest marking on the planet. In 1908 the north temperate zone was bright and broad, forming a marked contrast with the adjacent north temperate belt. This

year it has faded considerably in brightness, its position being marked by two narrow faint zones, and there is an almost uniform tone over the greater part of the northern hemisphere, the north temperate belt being just apparent on some of the negatives.

The brightest part of the disc is now undoubtedly the equatorial zone, which at the opposition of 1908 was slightly, if any, brighter than the north and south temperate zones, then about equal.

The Great Red Spot and its environs were photographed in February 1908, and also at the present apparition. The photographs of 1908 show the mottled appearance of the south temperate zone in the vicinity. This is not so apparent in the 1909 results, but the complicated shadings round the "Bay" are well shown.

The south temperate belt seems to be more easily photographed when the Red Spot is absent from the disc, becoming narrow and irregular as it approaches the spot. A photograph taken on March 21, 1909, shows a curiously irregular outline of the south polar regions, the "Bay" being visible on the preceding limb.

It is intended to measure the position of the belts micrometrically, as there seems to have been some shift in position during the last year. The photographs are hardly suitable for reproduction owing to the small size of the disc, the lack of contrast being accentuated by enlargement, but the definition obtained is often very fair, and measured drawings made from the negatives would have a distinct value as a check on drawings made at the eyepiece.

Observations of Jupiter's Galilean Satellites, January–June 1908. By R. T. A. Innes. (Plate 20.)

N.B.—Unless otherwise stated, all times are Greenwich Times, *i.e.* Greenwich Mean Times, but the now useless word "mean" in this connection is left out.

The observations which follow were made with a 9-inch Grubb refractor and a positive eyepiece giving a power of 700. With this power the disks of the four Galilean satellites were always visible, whilst with a power of 400 or 500 such was not the case. The observations are divided into three parts—

(*a*) phenomena, transits, occultations, and eclipses;
(*b*) sizes;
(*c*) surface markings, etc.

The method of observing a phenomenon was to note the instants of first contact with the limb of Jupiter, one-quarter phase, central phase, three-quarter phase, and last contact.

After preliminary experience it was found advisable to read the chronometer by eye, so as to make the time quite independent of bias; in other words, I soon learned that from one phase to another for satellite I took about 40 seconds; if, for

instance, a contact had been noted at x seconds, I knew the next phase was due at $x+40$ seconds, so that when $40+x$ seconds had elapsed, or nearly so, I felt it was time to make a further record: to avoid this bias, counting the seconds was given up, and the chronometer was read by eye instead. When an observation was completed, the mean interval between the observed phases was derived, and with this interval all the phases were reduced to central phase, much in the same way as a star-transit over five wires is reduced to the central or mean wire. The probable error of the mean central phase was derived by least squares. A glance at the observations shows that this probable error is not large, and is even considerably less than the probable errors of eclipse observations. But the observations are affected by irradiation and perhaps other disturbing causes; thus a complete shadow-transit always occupies less time than a satellite-transit. The duration of a phase, even for J I, varies very considerably on different evenings. It is obvious that the greater the irradiation, the longer are the durations of a phase and of a complete transit. Consequently the diameter of Jupiter found from complete transits will be larger for satellite-transits than for shadow-transits, and may even differ from one night to another, or from one satellite to another for satellite-transits. Shadow-transits will give the same diameter, irrespective of the albedo of the satellite. This uncertainty is relatively not very great. A transit of J I takes about $2^h\ 17^m$, or 8220 seconds, the uncertainty being about 5 seconds, or one part in 1600, or about $0''{\cdot}02$ in the diameter of Jupiter.

At quadrature (following opposition) it was found at entry of the shadow of a satellite that the shadow was so elongated E. and W. that the first and last contacts could only be usefully observed, whilst at egress a similar elongation, but N. and S., created even greater difficulty: thus for J II the shadow at egress in quadrature becomes so small and linelike that it is quite difficult, if not impossible, to note the time of last contact with certainty.

The best way to measure the precision of such observations as I have made is to compare them with independent observations of the same phenomena made elsewhere, but it is very necessary for this purpose that sufficient magnifying power to show the disk of the satellite should be used. With my shortsight, in itself a considerable magnifier, I find a power of 700, or 80 to the inch, about the minimum. I asked the co-operation of some astronomers, and gratefully record my obligations to them for the observations they sent me. I give a full list of the doubly observed phenomena. It will be seen that in no case was a power of over 30 to the inch used. Such a power might indeed reveal the disks of the satellites on a 9-in., but not in such a way as to be useful.* I do not, for this reason, consider that the independent observations yield a good measure of the attainable precision.

* Since the above was written, the Rev. T. E. R. Phillips and other observers have independently recommended the use of high powers (see *Mem. B.A.A.*, xvi. pp. 34–35).

1908 Feb. 10.—J I. Transit Egress—

Uccle. Mons. G. Van Biesbroeck. 15-inch ; power 360.

	Uccle. h	m	s	Johannesburg. h	m	s	
1st contact .	10	0	45	10	0	10	½ weight
¼ off . . .					1	16	
Middle-phase .		2	19		2	16	
¾ off . . .					2	58	
Last contact .		3	48		3	54	
Mean	10	2	17·8	10	2	7·5±1·9	

1908 Feb. 13.—J II. Occultation Ingress—

Utrecht. Mr v. d. Bilt. 10-inch. Satellite not seen as sharp disc.

	Utrecht (good seeing). h	m	s	Johannesburg (poor seeing). h	m	s	
1st contact .	11	24	21	11	24	14	
¼ phase . .					24	47	
Middle-phase .		25	53		25	44	
¾ phase . .					27	5	
Last contact .		28	7		28	48	double weight
Mean	11	26	7·0	11	26	11·9±6·7	

1908. Mar. 18.—J I. Transit Egress—

Kasan. Engelhardt Observatory. Mr. V. Baranof. 12-inch ; power 148.

	Engelhardt. h	m	s	Johannesburg. h	m	s
1st phase . .				6	54	21
2nd ,, . .					55	15
3rd ,, . .					55	56
4th ,, . .	6	58	11		56	39
5th ,, . .		59	0		57	44
Mean				6	55	59·1±1·9

1908 April 5.—J I. Transit Egress, Kasan—

	Engelhardt. h	m	s	Johannesburg. h	m	s
1st phase . .				5	54	49
2nd ,, . .	5	56	0		55	53
3rd ,, . .		56	35		56	51
4th ,, . .		58	27		57	20
5th ,, . .		59	20		58	13
Mean	5	57	11·5	5	56	35·5±2·5

1908 April 12.—J I. Transit Ingress, Kasan—

	Engelhardt. h	m	s	Johannesburg. h	m	s
1st phase . .	5	31	12	5	31	52
2nd ,, . .		32	28		32	27
3rd ,, . .		33	24		32	58
4th ,, . .		34	18		33	49
5th ,, . .		35	12		34	48
Mean	5	33	19·7	5	33	10·9±2·8

Egress J I.

	Engelhardt. h	m	s	Johannesburg. h	m	s
1st phase . .	7	47	30	7	47	4
2nd ,, . .		48	30		48	21
3rd ,, . .		49	36		49	13
4th ,, . .		50	22		49	58
5th ,, . .		51	16		51	13
Mean	7	49	28·3	7	49	13·4

1908 May 5.—J I. Transit Ingress, Kasan—

	Engelhardt. h	m	s	Johannesburg. h	m	s
1st phase . .	5	42	24	5	44	3
2nd ,, . .					45	1
3rd ,, . .		44	48		45	31
4th ,, . .		45	56		46	5
5th ,, . .		46	46		46	53
Mean	5	44	44·7	5	45	30·9±2·2

1908 May 12.—J I. Transit Ingress, Kasan—

	Engelhardt.			Johannesburg.		
	h	m	s	h	m	s
1st phase . .	7	39	58	7	39	31
2nd ,, . .		40	34		40	5
3rd ,, . .		41	26		41	5
4th ,, . .		42	14		41	45
5th ,, . .		43	10		43	22
Mean	7	41	28	7	41	14·7±5·0

It must be confessed that these comparisons are not very favourable; Kasan, however, used the very low magnification of 12 to the inch.

Of the conjunctions, the following comparisons are available :—

1908 Feb. 24.—J I. before J II.—

	Conjunction.			
	h	m	s	s
Potsdam, 20-in., power 450, Herr Hartmann	8	45	32·0	±5·0
Jhburg., 9-in., ,, 700	8	45	23·4	±2·7
Poulkowa, Mr. Kostinsky	8	44·2		

1908 April 17.—J III. to S. of J II.—

Kasan, 12-in., power 148, Mr. Baranof .	6	58	11·0	
Jhburg., 9-in., ,, 700, ...	6	58	6·9	±3·6

At Johannesburg there was no occultation, the least distance of limbs being 0″·15; but Mr. Baranof, using the power of 148, saw the satellites in contact for over five minutes.

In the following table no weight is attached to observations in [ ].

Date 1908.	Phenomenon.	G. T. of Mean Phase Deduced from I		2		Middle.		4		5		Concluded Time of Mean Phase.				Mean Quarter Duration.	Estimated Latitude.	Definition.	Remarks.
		m	s	m	s	m	s	m	s	m	s	h	m	s	p.e.	s	°		
Jany. 11	I. Sh Eg	15	33	15	34	15	30	15	36	15	32	8	15	33·0	±1·0	48·7			Time taken to nearest 5 secs. only.
18	I. Tr Eg	23	47	23	17	23	6	23	37	23	47	10	23	28·0	±8·5	50·0			
23	II. Oc Eg		—	38	20	38	22	38	14	38	20	7	38	19·0	±1·8	28·3			Time taken to nearest 5 secs. only.
29	III. Tr In	4	27	3	49	4	23	3	50	4	26	10	04	10·8	±8·8	76·7	−5	4	Shadow seen to S of III. but only as a fine crescent, it being nearly totally eclipsed.
		3	51	3	59	4	23	3	41	4	7	10	04	0·0	±4·8	86·0			Record confused—a 2nd interpretation.
Feby. 2	I. Oc In	41	55 *	41	66 *	41	66 *	41	50	41	53	8	41	56·3	±1·1	47·4			Double weight given to phases 4 and 5.
3	I. Tr In	0	49	0	34	0	35	0	31	0	49	6	00	39·6	±2·7	54·5		4	
	I. Sh In		Cloud		Cloud	10	25 *	10	5	10	15	6	10	13·1	±4·1	49·7		4	
	I. Tr Eg	17	35	17	35	17	50	17	19	17	35	8	17	35·3	±3·3	67·0		5	
	I. Sh Eg	25	31	25	29	24	57	24	54	25	31	8	25	16·5	±5·6	61·7		4	
8	II. Tr Eg	8	15 *	8	32	8	15	8	28	8	23	6	08	23·5	±3·8	71·0		4	
	II. Sh Eg	38	16	38	26	38	17	38	3	38	30 *	6	38	17·5	±5·0	61·7		4	
10	I. Tr In	45	3 *	44	50	44	47	44	54	44	59	7	44	53·5	±1·9	49·0	0±	3	
	I. Sh In	3	42	3	27	3	42	3	29	3	40 †	8	03	36·5	±2·5	45·3		2 – 3	
	I. Tr Eg	1	59 *	2	10	2	16	2	4	2	5	10	02	07·5	±1·9	54·3		2	
	I. Sh Eg	19	62	19	60	19	55	19	54	19	68 *	10	19	58·9	±1·4	33·2		2 – 3	Phase 5, clouds.
13	II. Occ In	26	31	25	55	25	44	25	57	26	31 †	11	26	11·9	±6·7	68·3	0±	4	Poor, II. is very small.
15	II. S I	23	15	23	43	23	22	23	20	23	38 †	6	23	29·2	±3·6	48·2		4	Phase 1, ⅛th. on.

* Half weight.

† Weight 2.

Date.	Phenomenon.	G. T. of Mean Phase Deduced from. 1		2		Middle.		4		5		Concluded Time of Mean Phase.				Mean Quarter Duration.	Estimated Latitude.	Definition.	Remarks.
1908.		m	s	m	s	m	s	m	s	m	s	h	m	s	p.e.	s	°		
Feby. 15	II. T E	24	51	24	52	24	49	24	49	24	54	8	24	51·0	±0·6	57·2		2–3	
	II. S E	14	20	14	18	14	9	13	56	14	41	9	14	16·8	±4·9	49·8		3–4	
16	III. O I	5	58	6	35	6	0	5	39	6	54†	6	06	20·2	±9·7	74·4	+3	3	Round. Recorded minutes looking erroneous, a new reduction is attempted, altering phases 1, 2 and 4 by 1 min. each.
	III. O I	6	8	6	2	6	1	6	12	5	59	6	06	04·5	±1·6	101·8		3	
18	I. O I	35	55	35	40	35	45	35	45	35	50	6	35	47·1	±1·8	56·5		3	
	I. E E	23	45	23	32*	23	45	23	48	23	47	9	23	45·1	±1·6	43·0		4	Phase 1, first speck of light at 9h 22m 19s was excellent, the remaining phases were poor, as def. was too bad to let the disc be seen.
19	I. T E	12	49	12	64	12	55	12	49	12	65	6	12	56·4	±2·3	39·3		2–3	
	I. S E	42	25	42	12	42	29	42	36	42	1	6	42	20·6	±4·2	46·2		2–3	
22	IV. S E		—	36	18	36	22	36	6	36	22	6	36	19·8	±3·5	66·0	−10	5	
	II. T I	48	41	48	36	48	41	48	43	48	55	7	48	43·2	±2·1	55·3	+2	4	
	II. S I	59	14†	59	23	59	30	59	13	59	14†	8	59	17·4	±2·0	51·5		3–4	
	II. T E	41	22	41	11	41	8	41	11	41	23	10	41	15·0	±2·1	50·2		2–4	
24	II. E E	58	58	58	52	58	56	58	55	58	55	6	58	55·1	±0·6	62·3		2	First speck at 6h 56m 53s very good.
25	I. O I	21	21	21	14	21	19	21	4*	21	32	8	21	19·6	±2·7	47·2	0	3	Round.
26	I. T I		Cloud	41	38	41	51	41	44	41	65	5	41	49·4	±4·0	(48)	−2	3	Poor, through passing clouds.
	I. T E	58	14	58	18	58	13	58	0*	58	12	7	58	12·6	±1·7	52·7		3	
	I. S E	35	36		Cloud	36	7	36	1	36	32*	8	36	00·3	±6·9	(50)	−2	3	Poor, through dense haze.
27	III. S E	37	44	37	70	37	59	37	44	37	64	5	37	56·6	±3·5	(60)	−1	5	Very poor obsns.
	I. E E	47	41	47	37	[47	58]	47	41	47	37	5	47	38·6	±0·8	47·0		4	First speck of light 5h 46m 5s is perhaps 1s late.

Mar.	2	II.	O	I	8	49	9	10	9	6	9	3	8	58	5 09 01·0	±2·4	44·0	0	5 Very poor obsns.
	4	I.	T	I			28	23	28	5	28	11	28	22	7 28 15·2	±2·9	45·8	0	4 Poor, haze.
	5	I.	E	E	42	37	Cloud								7 42 37·3		(45)	0	4 Passing cloud, clear for a few seconds, when first speck of light appeared at 7h 41m 07s·3, which is perhaps 1 sec. late.
	9	II.	O	I	28	18	28	28	28	21	28	12 *	28	17 *	7 28 20·4	±1·7	31·0	0	4 Poor, thick haze at end.
	11	II.	S	E	20	10	19	58	19	41 *	19	49	20	20	6 20 01·7	±3·7	51·2	+2	4 Haze.
		I.	T	I	15	18	15	16	15	13	15	11	15	23	9 15 16·0	±1·3	33·3	0	3
		I.	S	I	10	47	10	35	10	23 *	10	33	10	49	10 10 39·1	±2·8	48·5		4
	12	III.	T	I	14	32	14	35	14	61 †	14	13	14	54 †	6 14 44·1	±5·4	121·2	−3	2
		I.	O	I	22	47	22	62	22	66	22	57	22	52	6 22 56·9	±2·4	32·3	+1	2
		I.	E	E	37	22	37	20	37	17	37	20	37	23	9 37 20·5	±0·5	40·3		4 First speck at 9h 36m 10s·6. Phases by disc, not magnitude.
		III.	T	E	50	3	50	1	50	1	50	0	50	4	9 50 01·7	±0·3	90·5	−5	4
		III.	S	I	0	60	0	56	0	66 *	0	45 *	0	69	10 00 59·7	±2·7	97·0		5
	13	I.	S	I	39	3	39	3	39	15	39	6	39	10	4 39 9·7	±1·6	36·0	0	4 Daylight.
		I.	S	E	52	53	52	66	52	72	52	2	52	67	6 53 02·3	±2·7	53·0		4
	18	IV.	E	I			39	2	41	5	43	5			5 41 03·9		see note	+10	2 Last speck at 5h 50m 53s·2, good within 2 or 3 secs. Phases 2, 4 uncertain. See note.
		II.	S	I	7	3	7	15	7	17	7	0	7	15 †	6 07 10·7	±2 1	45·2		3
		II.	T	E	55	57	56	3	55	56	55	51	56	8	6 55 59·1	±1·9	48·2		3
	19	I.	O	I	11	36	11	29	11	33	11	26 *	11	38	8 11 32·9	±1·4	41·5	0	1 Dimming off of Jupiter's disc very apparent.
		III.	T	I	49	32	49	18	49	20 †	48	47	Clouded		9 49 14·9	±5·0	(90)	−5	3 Poor, through thin cloud.
	20	I.	T	I	Cloud		Cloud		[31	28]	[31	43]	[31	58]	5 31·7		(45)		cloud Partial glimpses through cloud.
	23	III.	E	E	Note		32	46	32	36	32	45	too hazy		7 32 41·8	±2·4	120·0		2 First speck 7h 26m 34s G.T. — very gradual, haze perhaps 2 secs. too late.

* Half weight.

† Weight 2.

Date. 1908.	Phenomenon.	G. T. of Mean Phase Deduced from. 1 m	s	2 m	s	Middle. m	s	4 m	s	5 m	s	Concluded Time of Mean Phase. h	m	s	p.e.	Mean Quarter Duration. s	Estimated Latitude. °	Definition.	Remarks.
Mar. 27	II. E E			36	21	36	25	36	27	36	20	6	36	23·5	±1·5	30·0		4	First speck at 6h 34m 42s through cloud, and perhaps too late; phases by estimation of mag., disc not seen.
28	I. O I	29	17	29	14	29	15	29	12	29	20	4	29	15·6	±0·9	39 8	+2	2	Daylight.
	I. E E			57	26	57	50	57	62	57	26	7	57	31·9	±4·0	44·0			First speck at 7h 55m 18s·5 good.
29	I. S E	10	36	11	13	10	53	10	38	11	12	5	10	54·3	±5·3	52·7	0	1 – 2	See note.
30	III. O E			46	7	45	50	46	22	46	5	6	46	05·9	±4·3	90·0	+10	3	Shadow line ±0″·2 (NA = 0″·29).
April 1	II. T I	57	67	57	54	57	48	57	32	57	88	8	57	57·6	±6·3	59·0	0	5	Poor.
3	II. E E			11	22	11	5	11	14	11	32	9	11	18·3	±3·9	43·3		3	First speck at 9h 9m 20s·0. The gain of light during the following 10s was very gradual, Phases partly by estn., disc not seen clearly.
	I. T I	13	3	12	55	13	9	12	50	13	8	9	13	01·2	±2·4	42·2	0	3	
4	IV. E E			36	27	35	58	36	17	36	25	4	36	17·4	±4·5	140·0	+3	3	First speck at 4h 31m 54s±. Although the Sat. was held steadily from first speck, it was 14 secs. before I could be quite certain of the reality of the reapp'ce. Phases by disc, but poor.
	I. O I	20	34	20	38	20	36	20	36	20	36	6	20	36·5	±0·2	42·0	0	2	Very good obs.
	I. E E			52	34	52	32	52	35	52	35	9	52	33·6	±0·5	46·0		5	First speck at 9h 50m 42s·9. Phases by disc, but poor.
5	I. S I	52	14	52	21	52	31	52	12	52	23	4	52	20·3	±2·3	44·2		4	
	I. T E	56	26	56	41	56	51*	56	32	56	36	5	56	35·5	±2·5	48·5		3	High wind shaking instr. Poor.
	I. S E	4	33	5	3	4	57	4	46	4	52	7	04	50 1	±4·5	36·5		4	Do. Very poor. Almost guesses. Shadow now very small. I very yellow.

April	6	I.	E	E			21 22	21 43	21 35	21 48	4	21	37·2	±3·8	[45]		3	Daylight. First speck at 4h 19m 43s·5. Phases by discs.
	6	III.	O	I	53	23†	53 15	52 40	52 38	53 42	6	53	14·8	±3·0	84·5	+5	3	
	10	III.	S	E				30 7	29 22	30 7	5	29	52·2	±10·0	[90]	−3	4	
		II.	O	I	24	22	24 27	24 23	24 11	24 36	6	24	24·2	±2·5	52·5	−1	4	Phase 5 good.
	11	I.	O	I	12	13	12 18	11 55	11 12	12 58‡	8	12	34·3	±7·2	[42·5]		5	Very poor throughout.
	12	I.	T	I	33	18	33 10	32 58	33 6	33 22	5	33	10·9	±2·8	42·8	0	3	
		II.	S	E	7	8	7 9	7 8	6 59	7 18	6	07	08·7	±2·1	46·8	+3	3	
		I.	S	I	47	14	47 32	47 30	47 19	47 28	6	47	24·6	±2·3	37·2	−1	4	
		I.	T	E	48	30	49 4	49 13	49 15	50 5	7	49	13·4		[43]		5	I became visible on disc at 7h 45m.
		IV.	S	I	0	56	1 22	0 29		0 52	8	00	54·8	±7·3	112·5	−8	4	First phase was ⅛th on, shadow of IV was decidedly oblong along equator and fully twice the dia. of the shadow of I.
		I.	S	E	59	6		58 55		59 6	8	59	01·8	±1·9	42·0		5	Shadow small and very difficult.
	13	I.	E	E			16 56	16 51	16 48	16 59	6	16	53·6	±1·7	40·5		3	First speck at 6h 15m 48s·7. Phases by discs, but poor.
	17	III.	S	I	27	59·5	[29 38·5]	[30 42·5]	[31 41·5]	35 1·5	5	57	22·9		105·5	+1	3	Between first and last contacts the shadow was so oblong along the equator that the estimate of intermediate phases became erroneous. For the phases local sid. times less 9h are given.
	19	II.	S	I	56	20		oblong		56 20	5	56	19·6		45·25	+3	2	
		II.	T	E	13	6†	13 3	13 26	13 2	13 16†	6	13	11·0	±2·8	45·8		2	
		I.	T	I	[27	32]	27 10	26 55†	26 37	26 54†	7	26	54·4	±3·7	[45]	0	3	

* Half weight. † Weight 2. ‡ Weight 4.

Date.	Phenomenon.	G. T. of Mean Phase Deduced from. 1		2		Middle.		4		5		Concluded Time of Mean Phase.				Mean Quarter Duration.	Estimated Latitude.	Definition.	Remarks.
1908.		m	s	m	s	m	s	m	s	m	s	h	m	s	p.e.	m	″		
April 19	I. S I	42	48			oblong				42	48	8	42	48·2		47·2		3	Planet low.
	II. S E	43	4			43	14			43	4	8	43	07·4		33·2		3	Planet low. Shadow small. Difficult.
20	I. O I	35	43	35	40	35	40	35	37	35	45	4	35	40·7	±0·9	36·3	0	3	
	IV. O I	33	61	33	28	33	2	33	39	33	49	5	33	35·8	±6·7	119·2	+15	3	IV large and dim, and difficult to observe with accuracy.
	I. E E																	5	First speck 8h 10m 368·2, probably a few secs. late as definition was atrocious and too bad for discs.
21	I. S E	23	2	23	8	23	4	23	0	23	10	5	23	04·6	±1·3	43·5	0	3	Shadow very small.
24	III. T I	49	32	50	0	49	39	49	24	50	12	4	49	45·4	±5·9	98·7	−5	2	See note on shape of III.
	III. T E	24	48	24	34*	24	54	24	26*	24	55	8	24	46·6	±3·4	102·7	−5	3	
26	II. T I	1	52	1	54	1	48	1	30*	2	15	6	01	54·0	±4·3	51·2	+2	4	Time read 5m wrong? If so, see next line.
												5	56	54·8					
27	I. O I	30	45	30	60	30	57	30	48	30	57	6	30	53·3	±1·9	43·5	0	4	Poor.
28	III. E E			32	24	32	21	32	1	32	14	3	32	15·0	±3·5	98·0		2	First speck 3h 27m 538·7 in full sunlight, phases by disc. See note.
	I. T I	50	28*	50	30	50	14	50	1	50	14	3	50	16·3	±3·5	41·0	0	3	Daylight.
	I. S I	5	26§	5	27	5	36	5	23	5	30§	5	05	28·4	±1·1	46·3			1 perhaps a few seconds late; 2-3-4, shadow oblong, E. and W. equ.
	I. T E	5	20	5	48*	5	38	5	24*	5	32	6	05	31·6	±3·2	41·5			
	I. S E	17	36	17	60	17	57	17	40	17	59	7	17	50·4	±3·4	[40]	0	4	V. dif. Shadow grows small rapidly just before contact. Jupiter in quadrature. Shadow probably elong. N & S.
	II. E E			12	44	12	42	12	29	12	51	6	12	41·2	±3·1	43·0			First speck 6h 11m 088·9. Full light about 6h 15m Phases by discs but dif.

April	29	I.	E	E			36	25	36	43	36	35	36	25	4 36 32·0	±3·0	[42·0]		3 Firs. speck 4h 34m 44s·3.
		IV.	S	E	41	5	[40	40]	[40	25]	[40	8]	41	6	6 41 05·5		101·5	−5	3 Shadow large until close on int. cont., when it grew rapidly small and difficult to see.
May	5	I.	T	I	45	21	45	40	45	31	45	26	45	35	5 45 30·9	±2·2	39·0	0	4
		I.	S	I	0	57			oblong				0	57	7 00 56·9		40·5		4
		III.	E	E			28	36	28	31	28	38	28	28	7 28 33·6	±1·6	82·5		4 First speck at 7h 25m 10s·0. Disc obs. very poor.
	6	I.	E	E			32	11	32	12	32	14	32	9	6 32 11·6	±0·7	43·0		4 First speck at 6h 30m 19s·4. By discs.
	7	I.	S	E	42	5			42	15			42	6	3 42 08·8	±2·2	35	0	2 Most difficult as shadow is so minute. Daylight.
		IV.	O	E	36	29	36	4	36	14	36	24	36	18	4 36 17·7	±3·0	145	+20	2 IV. very dim and difficult. Shading on limb of J distinct.
	12	II.	O	I	2	46	2	42	2	42	2	31	2	55	6 2 44·0	±2·6	44·4	0	2
	12	III.	O	E	27	9	27	8	27	44	27	9	26	21	6 27 05·6	±8·9	118	+3	3
		I.	T	I	41	21	41	0*	41	5*	40	50*	41	32	7 41 14·7	±5 0	55	−1	4 Planet low, lower limb blue from atmos. refraction.
		III.	E	I											7 48 33				5 Last speck 7h 53m 21s. Poor ± 5 secs. Mid. ph. very poor.
	13	I.	O	I	53	33	53	40	53	52	53	39	53	34	4 53 39·6	±2·3	44·2	+1	2
	20	I.	O	I	51	8	50	58	51	1	51	4	51	2	6 51 2·4	±1·1	47·5	0	4
	21	I.	S	I	19	10	19	20	19	28	19	23	19	7	5 19 16·0	±2·7	47·8		4
		II.	S	I			46	9	46	54*			46	30	5 46 26·5	±8·1	[45]	+5	5 Very poor.
	22	I.	E	E			51	50	51	53	51	64	51	49	4 51 56·5	±2·7	46·7		4 First speck 4h 50m 58s·1. Phases by disc ; poor.
	23	III.	S	E	26	51	26	4	25	41	26	16	26	38	5 26 17·6	±8·3	90·0	+3	4 Difficult.
	28	II.	T	I			3	49	3	42	3	43	3	61	6 03 49·1	±2·9	48·0	+2	4
		I.	T	I	5	38	5	16	5	15			5	39	6 05 27·2	±4·4	45·4	−1	4

* Half weight. † Weight 2. § Weight 3.

Date.	Phenomenon.	G. T. of Mean Phase Deduced from. 1		2		Middle.		4		5		Concluded Time of Mean Phase.				Mean Quarter Duration.	Estimated Latitude.	Definition.	Remarks.
1908.		m	s	m	s	m	s	m	s	m	s	h	m	s	p.e.	s	°		
May 29	I. O I	20	19	20	16	20	21	20	14			3	20	17·6	±1·1	37·5		2	Sun shining on telescope.
30	III. T E	2	6	2	29	2	31	1	57	2	39	5	02	20·5	±5·3	93·5	−2	4	
	II. E E			48	46	48	38	48	39	48	45	5	48	41·9	±1·4	44·0		3	First speck 5h 47m 16s., good.
	III. S I	54	69	54	27	54	26	54	70	54	28	5	54	44·1	±6·9	125·2	0	3	Shadow oblong.
June 1	IV. T I	52	53†	52	4	51	3	50	38	53	22†	3	52	18·5	±20·0	124·0	−10	3	Transited broad belt. IV was brighter than belt at entry, later it was visible as a grey spot on belt.
6	I. T E	50	50	50	48	50	38	50	26	51	14	4	50	47·3	±5·3	49·3		3	
	III. T I	40	38	39	56	39	59	39	2	41	20	5	40	13·0	±16·4	102·0	−2	4	
	I. S E	49	37	50	11	49	54	49	31	50	55	5	49	52·0	±5·2	54·0		4	Poor.
10	IV. E E		...		...		...		...		...		...			...		...	Seen 10th mag. at 4h 49m 13s. between passing cloud—probably some minutes late.
14	I. E E			7	50	8	15	8	11	8	10	5	08	06·7	±3·8	[45]		5	First speck at 5h 5m 19s·6 through thin cloud and probably late. Disc phen. very poor.
15	II. T E	54	31	54	23*	54	39	54	23*	54	31	3	54	31·0	±2·0	71·8	+1	3	
17	III. O E	43	29‡	43	45‡	44	2†	43	59	44	19	3	43	46·3	±5·2	[100]	+3	3	The satellite began to disappear in eclipse before it fully emerged.
	III. E I			48	50	47	48	48	13			3	48	17·1	±12·1	,,		4	Last speck at 3h 51m 38s. Disc obsns. obviously poor as the satellite was never wholly visible.

* Half weight. † Weight 2. ‡ Weight 4.

Extra Notes.

Definition. 1 means excellent seeing.
3 means fair seeing.
5 means very bad seeing.

The observations were made in sidereal time, but the times of the reduced phases are in Greenwich time; the mean of the five phases may, for that reason, differ by a fraction of a second from the given mean. The mean quarter duration, *i.e.* the time from one phase to the next, is given in sidereal time.

1908 January 29 and February 16, J III.—On both occasions some of the recorded times seem to be a minute in error—the second lines include the effect of plausible but possibly erroneous corrections to the minutes; the net results differ by 10 seconds and 15 seconds, corresponding to alterations in the geocentric positions of 0″·03 and 0″·04 respectively.

1908 March 18, J IV, Eclipse Ingress.—The final disappearance of the satellite was very slow, and proved that the estimates of a quarter, a half, and three-quarters of disc covered were largely in error. The fading away of J IV was so gradual that I should estimate the probable error of the observation to be six times as great as for J I. The shape of the illuminated disc of J I in partial eclipse is very easy to see, but J II is too small, J III is too irregular in shape, and J IV is too dusky for a 9-inch aperture.

1908 March 23.—The very gradual loss and gain of light in eclipses of J III and J IV prove the presence of penumbral effects. In the cases of J III and J IV I feel sure that observations of the contact phenomena (transits, shadow-transits, and occultations) are more precise.

1908 March 29, J I, Shadow Egress.—

The chronometer times actually observed were

1st phase	7^h	27^m	37^s (very good obs.)
2nd ,,		29	7
3rd ,,		29	40
4th ,,		30	17
5th ,,		31	43

from which

(1+5)/2	7^h	29^m	40^s
(2+4)/2		29	42
3		29	40

—results which look better than the separate results of the reduced phases. (*P.S.*—No doubt the elongation of the outline of the shadow began to get troublesome as Jupiter approached quadrature.)

1908 April 28, J III, Eclipse Egress.—In sunlight; J IV was only seen with difficulty.

3^h	28^m	26^s	J III = J IV in light-magnitude.
3	31	44	J III = J I ,, ,, ,,

Comparisons with the Ephemeris.

The following table shows at a glance the run of the errors, but it is important to remember that, with the exception of the eclipses, the phenomena are only predicted to the nearest minute. When, however, the minute of the *Nautical Almanac* differed from that of the *Connaissance des Temps* or the *American Ephemeris*, the mean of the three was taken. Such cases are indicated by the addition (3).

The systematic run of the errors of transit and shadow phenomena is due to the fact that the predictions are for Jupiter's whole disc, and no allowance is made for the shadow-edge or the turned-away illuminated edge. These effects are, of course, most conspicuously seen in the observations of J I. The constant errors are partially due to errors of the ephemeris, but in the case of J I more largely due to the value adopted for Jupiter's diameter. As indicated above, the value of this diameter will depend on the power used, and will differ from the various classes of phenomenon.

SATELLITE I.

Obs.—N.A.

1908.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.	Angle. Sun-Jup.-Earth. °
Jan. 11								−1·5	3·8
18						−4·5			2·4
Feb. 2			+0·9						0·8
3					−0·3	−3·4	+2·2	−2·7	1·0
10					−0·1	−2·9	+1·6	−3·0	2·5
18		−0·75	−0·2						4·3
19						−2·1		−3·7	4·4
25			+0·3						5·4
26					+1·1 C. de T.	−2·8		−4·0	5·6
Mar. 4					+1·3				6·7
5		−0·52 clouds							6·8
11					+1·3		+1·7		7·8
12		−0·73	+0·6(3)						8·0
13							+1·2	−5·0	8·2
19			+0 5						8·7
20					+2·7				8·8
28		−0·69	+0·3						9·7
29								−5·1	9·8
Apr. 3					+3·0				10·0
4		−0·63	−0·4						10·2
5						−1·4	+1·3	−6·2	10·3

SATELLITE I.—*continued.*

Obs.—N.A.

1908.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins	Shadows. I. mins.	Shadows. E. mins.	Angle. Sun-Jup.-Earth.
Apr. 6		−0·52 daylight							10·4
11			−1·$I_{(3)}$						10·6
12					+3·2	−0·9	+1·4	−7·0	10·6
13		−0·59							10·6
19					+2·9		+1·8		10·8
20		−0·50 poor	−0·3						10·9
21								−6·9	10·9
27			−0·1						10·9
28					+3·3	−1·5	+1·5	−6·2	10·9
29		−0·60							10·9
May 5					+2·5		+1·9		10·8
6		−0·46							10·8
7								−5·9	10·8
12					+2·2				10·5
13			−0·3						10·5
20			+0·0						10·2
21							+2·3		10·2
22		−0·40							10·0
28					+1·5				9·6
29			+0·3						9·5
June 6						−2·2		−6·1	8·8
14		+0·29							8·2

SATELLITE II.

1908.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.
Jan. 23				−1·7				
Feb. 8						−4·6		−3·7
13			+0·2					
15						−3·2	+1·5	−3·7
22					+0·7	−3·8	+1·3	
24		−0·62						
Mar. 2			+1·0					
9			+0·3					
11								−6·0
18						−2·0	+0·2	
27		−0·37						

SATELLITE II.—*continued.*

1908.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.
Apr. 1					+1·0			
3		−0·62						
10			+0·4					
12								−6·9
19						−1·8	+1·3	−7·9
26					+1·9			
28		−0·55						
May 12			−1·7					
21							+0·4	
28					−0·2			
30		−0·37						
June 15						−1·5		

SATELLITE III.

1908.	Eclipses. I. mins.	Eclipses. E. mins.	Occultations. I. mins.	Occultations. E. mins.	Transits. I. mins.	Transits. E. mins.	Shadows. I. mins.	Shadows. E. mins.
Jan. 29					0·0			
Feb. 16			+1·1					
27								−5·1
Mar. 12					+2·7	−4·0	+1·0	
19					+2·2			
23		−0·28						
30				−1·9				
Apr. 6			+0·2					
10								−9·1
17							+1·4	
24					+4·2	−2·2		
28		+1·32 sunlight						
May 5		−1·47						
12	+0·97			−1·9				
23								−10·7
30						−0·7	+1·7	
June 6					+3·3			
17	+0·33			−1·2				

SATELLITE IV.

1908.	Eclipses. I.	Eclipses. E.	Occultations. I.	Occultations. E	Transits. I.	Transits. E.	Shadows. I.	Shadows. E.
	mins.	mins.	mins.	mins.	mins.	mins.	mins.	mins.
Feb. 22								−14·7
Mar. 18	+4·03							
Apr. 4		−2·50						
12							−3·1	
20			−1·4					
29								−18·9
May 7				+0·3				
June 1					−0·7			
10		−0·32						

The following observations were made by Mr. Worssell at Braamfontein, near Johannesburg, with a 3-inch refractor. Mr. Worssell determined time on each evening by sextant observations.

		1st Speck seen.				O—C.	
			h	m	s	s	
March	28	Ec. Re. I.	7	55	21	−39	Definition good. Satellite looked for too far from planet.
April	3	Ec. Re. II.	9	9	44	−15	Satellite first seen very close to edge — observation probably late. Definition fair.
	13	Ec. Re. I.	6	15	13	−27	Smoke from Braamfontein station interfered. Definition poor.
	20	Ec. Re. I.	8	10	48	−18	Definition medium. Planet $\frac{1}{3}$ in field.
May	5	Ec. Re. III.	7	25	28	−64	Definition good. Edge of planet in field.
	6	Ec. Re. I.	6	30	22	−25	Definition bad. Lost count? Edge of planet in field.

Conjunctions of Satellites, etc.

Distances of centres are calculated for $\Delta = 5{\cdot}202$. Diameters assumed are: — J I = 0″·80; J II = 0″·71; J III = 1″·14; J IV = 1″·02.

n. = near half of orbit
f. = far part of orbit
} when attached to a satellite number.

1908.
Feb. 24. J II f. passed behind and to the N. of J I n. The record in local sidereal time is as follows:—

		h	m	s	
One diameter apart ±		8	47		
1st contact at	295°±	8	49	38	
☌	90°±	8	51	11	II. was ⅛ covered.
	45°±	8	51	58	
Last contact at	60°±	8	52	11	
Well separated		8	53		
One diameter apart		8	55		

Fine observations; none of the four phases will be in error by 5 secs. The times of mean conjunction computed from the estimated angles are:—

		h	m	s	
1st	295°	8	50	51	Mean $8^h\ 45^m\ 23^s{\cdot}4 \pm 2^s{\cdot}7$ G.T.
☌	0	8	51	11	
	45	8	51	1	Least distance of centres 0″·52.
Last	60	8	51	1	

It looked as if there might be an eclipse of J II by the shadow of J I, but a watch until $9^h\ 22^m$ G.T. was unfruitful.

N.B.—In this and similar observations, N. and S. refer to Jupiter's equator as estimated by its cloud-belts, *i.e.* the cloud-belts were assumed to be due E. and W. The record and reductions of further observations are abbreviated; the above example will indicate the procedure adopted both in observation and reduction.

Feb. 27. Partial occultation of J II f. by J III n. J II passed to the N. of J III, and about ⅕ of its diameter was covered at time of conjunction.

		h	m	s	
1st contact	330°±	10	22	17	Loc. sid. time.
☌	0	10	23	37	Double weight.
Last contact	30°	10	25	10	
	Mean	10	23	$39{\cdot}2 \pm 2{\cdot}1$	$= 10^h\ 5^m\ 59^s{\cdot}3 \pm 2^s{\cdot}1$ G.T.

Least distance of centres 0″·77.

A very good observation (Def. 2).

Mar. 16. J I n. passes S. of J III f. Observed at first through dense haze. Five phases observed (Def. 4).

Reduced time ☌ $8^h\ 30^m\ 37^s{\cdot}0 \pm 4^s{\cdot}4$ G.T.
Least distance of centres 1″·37
,, ,, between limbs = ½ of diameter of J I.

The motion in latitude was considerable, probably J III moving S.

1908.
Mar. 23. J II n. passes S. of J III f. The least distance of limbs was equal to one diameter of J III, therefore distance of centres 2″·06. Time of ♂ = $9^h\ 15^m{\cdot}5 \pm 0^m{\cdot}2$, G.T.

April 4. Ec. of J II by J I predicted for $9^h\ 51^m$; watch kept from $9^h\ 40^m$ to $10^h\ 0^m$; no appreciable dimming seen. J II was all the time much brighter than J IV.

8. Eclipse of J I by J II predicted for $6^h\ 31^m$; watch kept from $6^h\ 25^m$ to $6^h\ 40^m$; no dimming observed. J I was much brighter than J II.

This evening J II passed in front of J I, which was slightly N. of J II at first contact, and very slightly S. at last contact.

	h	m	s	
1st contact at 280°	7	21	17	Local sid. time.
As one from $7^h\ 23^m\ 10^s$ to $7^h\ 25^m\ 33^s$		24	21	
Last contact at 95°		28	41	
	7	24	53	
or	4	26	29	G.T.

The p. e. is about 10 secs, or about o″·04, in the geocentric place. By calculation from the contact angles, at time of ♂ the centre of J I was about o″·03 N. of the centre of J II.

10. Eclipse of J II by J III predicted for $7^h\ 28^m$. This time is obviously erroneous, as J II had occultation ingress at $6^h\ 24^m$. The time of eclipse should be very closely $6^h\ 7^m$. I watched from $5^h\ 55^m$ to occultation ingress; at $5^h\ 59^m$ J II looked dim, but no central and, as far as I could see, no partial eclipse took place. A line from J III to Jupiter's centre passed to the S. of J II.

1908.
April 12. J II and J IV in ♂ (Jupiter's eq.) at $7^h\ 57^m$. J II is about 8″ N. of J IV.

15. J II n. passed across J I nearly centrally.

	h	m	s			
1st contact at 100° on J I				10	9	48 Local sid. time.
Round from	10	11	45			
to	10	13	42	10	12	43 ,,
Estimated central	10	12	42			
Last contact at 280° on J I				10	15	31 ,,
						,,
Mean central ♂				10	12	41·0 ,,
or				6	46	18·4 G.T.

1908. By a squared paper diagram I find that at the time of ♂, the centre of J II was 0″·01 to the N. of the centre of J I. An uncertainty of 5 secs. in the time of ♂ corresponds to an error of 0″·02 in the relative geocentric positions of J I and J II (Def. 3).

April 17. J III n. passed to the S. of J II f. A graphical reduction of estimated position-angles with Jupiter's axis at 315°, 330°, 0°, 30°, and 45° gives local sid. time of conjunction, $10^h\ 32^m\ 24^s{\cdot}5 \pm 3^s{\cdot}6 = 6^h\ 58^m\ 06^s{\cdot}9 \pm 3^s{\cdot}6$ G.T. At time of ♂ (Jupiter's axis) the centre of J II was 1″·07 N. of the centre of J III; the limbs were estimated $\frac{1}{5}$ of the diameter of J II apart.

May 4. J III ♂ J II at $5^h\ 58^m{\cdot}6$ G.T. centre J III, about 2″·5 N. of centre of J II.

12. J III ♂ J I at $7^h\ 9^m$ G.T. centre J III, about 1″·8 N. of centre of J I.

Least distance of limbs = diameter of J I.

14. J I and J II shadows in mid-transit. J II is its diameter N. of J I.

16. 4^h G.T. J I nearly in ♂ with and N of J III.

28. ♂ J I S. of J II at $12^h\ 22^m{\cdot}4$ (J I overtaking J II, ♂ took place after entry on disc of Jupiter—there was always a separation. Nearest approach at $12^h\ 22^m{\cdot}4$ centre of J II N. of J I by $\frac{\text{I.} + \text{II.}}{2} + \frac{\text{II}}{3}$ = 0″·99.

June 1. ♂ of J I and J II at $6^h\ 10^m\ 19^s$ G.T. This conjunction was nearly central, viz.

1st contact J II touching J I at 85° on J I at $6^h\ 4^m{\cdot}0$
Last ,, ,, ,, ,, 280 ,, ,, $6^h\ 19^m{\cdot}4$

With bad seeing (4) the two satellites appeared as one for a space of 6 minutes. Assuming the sum of the diameters of J I and J II to be 1″·5, then at ♂ the centre of J II was 0″·19 N. of the centre of J I. I estimate the uncertainty of time of conjunction to be 20 seconds; as the relative position J II to J I only changed by 1″·4 in 15 minutes, this uncertainty corresponds to 0″·03 in their relative position.

In spite of bad seeing, a good observation.

Duration of Normal Contact Intervals and Resulting Diameters of the Satellites.

The method of determining the diameter of a satellite from the interval between first and last contact with its primary's disc is an old one, and theoretically it is a very simple one.

If λ is the latitude of the point on Jupiter's disc where a satellite enters or leaves, the distance between the positions of the

centre of the satellite at first and last contacts is very approximately = diameter $\times$ Sec λ. If an error of 1% may be tolerated, then cos λ may be taken as unity up to 8°, and the distance between the centre at first and last contact is equal to the diameter of the satellite.

The mean movements of the satellites along the x co-ordinate (Jupiter's equator) at the times of contact with Jupiter's disc are as follows:—

Satellite.	Movement in 10 mins. at $\Delta = 5{\cdot}202$.
I.	2″·714
II.	2 ·172
III.	1 ·726
IV.	1 ·304

The deviations from the mean are in this connection sufficiently small to be neglected, and will be entirely eliminated if the occultation observations are as numerous as the transits. It will be noticed that the actual distance of the Earth from Jupiter at the time of observation does not matter.

In practice, the duration of a contact interval is affected by several sources of error, viz. irradiation, which lengthens the interval by night and shortens it by day; defect of the telescopic image, which decreases with increase of aperture and magnifying power; and lastly, personality. But these errors are also found with micrometrical measures of the diameters, which in addition have errors due to the thickness of the wires and their illumination.

From the observed quarter-durations printed above, I have deduced the duration of normal contact intervals, and find them as in the following table. A few observations made under bad conditions and a few discordant results have been omitted.

	Duration of Normal Contact Intervals.	Resulting diameters ($\Delta = 5{\cdot}202$).
J I.	175 secs.	0″·792
J II.	198	0 ·717
J III.	401	1 ·154
J IV.	470	1 ·021

For the theoretical reasons given, it is fairly certain that all these numbers are too large. The probable errors are about one-fortieth of the quantities. The diameters found for J III and J IV are much smaller than those found by observers using the micrometer, but no allowable manipulation of the contact observations will give diameters so large as are the micrometrical ones; thus in the case of J III, my durations vary from 340 secs to 500 secs, but even the latter duration only gives a diameter of 1″·43. Professor Barnard observed five durations of J III in 1890 (*M.N.*, li. p. 550), viz. 329^s, $430^s(\frac{1}{2}w)$, $478^s(\frac{1}{2}w)$, 460^s and 304^s, giving a mean of 400^s and a diameter of 1″·151. The evidence in favour of the smaller diameters of J III and J IV is very strong.

Between 1908 February 24 and March 29 I made numerous estimates of the sizes of the satellites. It had been intended to estimate their relative diameters, but at the latter date it was found that the eye is to a certain extent influenced by the area of the object. By experiments with coins, it was further found that the error of estimate varies with the ratio of the diameters. Of these earlier observations, it will suffice to say that the order of size found was:—J III much larger than J IV, J IV much larger than J I, J I slightly larger than J II.

After 1908 March 29 I estimated the diameters of the satellites by means of a set of discs drawn in black on white, which varied in size by tenths from 0·3 to 1·0 (unit = 2^{cms}). With these artificial discs, estimates (30 in all) were made, from 1908 April 4 to June 2, at all times that any pair of satellites were sufficiently close together. Such estimates can only yield the relative sizes of the satellites, so that it is necessary to assume the diameter of one of them is known: if we take J III as being = 1″·154, the ratios found then give

J I = 0·82. J II = 0·69. J III = 1·15. J IV = 1·03.

In comparing these figures with those given above it would seem that, if anything, the diameter of J III is still found somewhat too large.

Markings on the Satellites.

At all times J I and J II were seen as uniformly lighted round discs; J III was often strikingly deformed, and J IV was too dim to be seen clearly with the light-grasp of the 9-inch. As in the other observations, a power of 700 was always used, and I do not consider, as I have already said, that a lower power would be useful. The difference between the pretty picture of Jupiter, its cloud-belts and four bright satellites, under powers of 200–300, and a power of 700, was remarkable; with the lower powers J IV appears as a tiny sparkling point, but with 700 it was seen as a large sombre disc of a hexagonal shape, due no doubt to dark markings which were beyond the light-grasp of the telescope. With 200 magnification, J II was star-like, but with 700 its disc, though small, was quite distinctly seen. The remarkable shape of J III was first noticed on 1908 Feb. 10, and between that date and 1908 June 6 about sixty observations were made. On nights when the irregular shape, bright spot or mark on J III were well seen, sketches were made; these have been reproduced by Mr. Wesley and arranged in order of age in days from superior geocentric conjunction. Mr. Wesley must not be blamed for the inartistic appearance of my sketches, which indeed are not meant to be pretty pictures, but to represent as closely, faithfully, and without any imagination, what I actually saw. After all, J III is but a small object in a telescope. My present interpretation of the observations is that J III is round, as is proved by its shadow, in

N

0·7 1908 April 14	0·9 1908 April 7	1·5 1908 April 22
1·7 1908 April 15	2·0 1908 May 14	2·3 1908 April 30
2·5 1908 April 23	2·7 1908 April 16	3·5 1908 March 12
3·5 1908 April 24	3·7 1908 April 17	3·9 1908 Feb. 27
4·0 1908 May 16	4·3 1908 May 2	4·7 1908 April 18
5·2 1908 March 28	5·9 1908 April 12	6·7 1908 March May

S

f p

III. SATELLITE OF JUPITER.

Sketches arranged in order of days since Superior Geocentric Conjunction.

transit, but that it has some very dark markings, which, however, are always on the following side, and do not transit its disc; that the satellite may rotate in the same time as it revolves (about 7·1 days), or it may rotate exceedingly slowly; the bright spot is not always marginal, but when it is, it is at a position-angle of about 30° from Jupiter's axis. The plausible interpretation that J III does not rotate (or does so very slowly), but has large librations, must await further and almost continuous observations.

The irregular shape of J III has often been noticed; thus Lassell (*M.N.*, x. p 113, and xvii. p. 67) says:

1850 Feb. 28. 11^h 48^m G.T. J III, oblong or irregular. (Age 5·8 days.)

Mar. 12. 11^h 25^m G.T. J III is approaching the f. limb of the planet. There is certainly a spot on the satellite's disc in the n.p. quadrant. Its disc also seems a little flattened on the f. side. (Age 3·6 days.)

1856 Oct. 5. 10^h 45^m G.T. J III and J IV seem oval. (Age 1·8 days.)

Observations of J III.

1908.

Feb. 10. 10^h G.T. J III decidedly elliptical, longest axis N. and S. Ellipticity 1 in 5. Outline irregular, is kidney-shaped. J I and J II round.

24. 7^h–9^h G.T. J III like Moon 9 days old, term. away from Jupiter—indistinct markings.

25. J III shape of Moon 8 days old.

27. J II is round; its surface is much brighter than that of J III, and about equal to the bright spot on J III's northern part. J II about equal to the N. part of J III. Whilst waiting for the conjunction of J II and J III on this evening, the irregular shape of J III attracted attention. It is most easily described as being shaped like a cottage loaf or a squat figure-of eight, the smaller flattened part being to the N. The N. end of the axis was tilted about 30° towards Jupiter. The S. part was the larger and was yellow; the N. part was bluish with a brilliant white speck, not unlike the polar cap of Mars, but it was not marginal. The sketch, necessarily rough, gives a fair idea of the appearance.

Observations after February refer to the third satellite, J III.

Mar. 5. Shadow of J III in mid-transit, is large and round; J III against sky is perhaps elongated N. and S., but definition is bad.

9. 6^h G.T. Slightly elongated N. and S. All discs clearly seen. J IV large and dim, an effect which is quite lost with a power of 120.

1908.

Mar. 10. 6^h G.T. Shaped like Moon 9 days old, convexity to Jupiter; northern spot and equator band glimpsed.

11. Nearly round; it has a dark line along lat. 40°, with a bright spot within (N. hemisphere).

12. 6^h G.T. Round; it has a bright white spot in the N.E. quadrant with a dark line. Both before and after transit this evening, round.

13. Nearly if not quite round, spot unchanged.

15. Round, or nearly so; no certain markings.

16. Very hazy. The effect of haze on the appearance of J I, J II, and J III (but not J IV) in reducing their apparent sizes indicates that, like Jupiter, the margins are not so bright as the centres of their discs.

18. 5^h 30^m G.T. Like Moon 8 days old, convexity to Jupiter; marking and spot seem to be more crowded to the N.E. (N.f.) than usual.

19. 6^h 30^m to 8^h G.T. Approaching transit is oval and cottage-loaf shaped, with bright N.f. spot on N. part of disc; is tilted 20 degrees away from Jupiter.

22. All four discs remarkably well seen. 5^h 30^m G.T. Near p. elongation is round, no markings certainly seen. 9^h G. T. J III round.

28. 4^h 35^m – 6^h 35^m G.T. Nearly round, perhaps a bit cut off on side next to Jupiter; bright spot and marking are barely visible, and are close to limb at 45° (N.f.).

29. 5^h – 7^h 45^m G.T. Round, no markings, certainly seen; perhaps J III is just a bit brighter at 45° (viz. on N.f. limb).

30. 6^h 47^m G.T. Round; no markings certainly seen.

31. 7^h 39^m G. T. Like Moon 8 or 9 days old with blunted horns, term. f.—*i.e.* away from Jupiter. No markings certainly seen.

April 1. 5^h 40^m G.T. Like Moon 8 days old, term. f., longer axis N. and S. to Earth's equator. Spot and mark visible by glimpses.

2. 5^h 30^m G.T. Nearly round.

3. 6^h G.T. Like Moon 11 days old, term. to Jupiter; spot and mark glimpsed; bad air.

4. 6^h G.T. Round; perhaps a bright spot on limb N.f. 45°, but quite uncertain.

6. 4^h 30^m – 6^h 30^m G.T. J III round.

7. 6^h. All but round; a dark mark encroaches at 45°. 7^h G.T. slightly oval, like Moon 11 days old.

8. 6^h. Nothing certain; round or nearly so; a dark indentation about 60°.

10. 6^h. Like Moon 8 days old, terminator to Jupiter.

12. 6^h. Very slightly oval, spot and mark seen with difficulty; the marking is nearly central.

13. 6^h. Round or nearly so.

1908.

April 14. $5\frac{1}{2}^h$. Like Moon 9 days old, term. away from Jupiter, spot and mark seen with difficulty in N.f. quadrant.
15. 5–6^h. Like Moon 8 days old, marking seen as on 14th.
16. Like Moon 10 days old, marking nearer equator.
17. Just past transit; in moments of good definition the cottage-loaf is seen with bright spot, but III is nearly round. The shadow of J III in central transit is perfectly round, and about the same size as III itself, but J III is apparently longer in its N. and S. length.
18. Nearly if not quite round; marking and spot probably, but not certainly seen.
19. 7^h G.T. Round, no marking or spot seen; good definition.
20. Round.
21. Like Moon 9 days old.
22. Kidney-shaped, but nearly round.
23. Kidney-shaped, N. part very small.
24. Mark and bright spot seen most distinctly before ingress, cottage-loaf appearance quite plain at times; when J III was half on the planet it looked like a close double-star, the stars being in contact, with the smaller component N.f. When one quarter on the disc of Jupiter, it appeared as a gray oblong spot, the longer axis being in 300°–120°, *i.e.* inclined 30° to the belt.
25. 5^h. Round, perhaps a dark mark across disc, but uncertain; certainly no bright spot visible.
27. Round, or nearly so.
27. 5^h. Perhaps elongated N. and S., no markings or spot seen.
28. $4^h\ 45^m$. Just out of eclipse, round; no marking or spot visible, definition extraordinarily good.
29. $4^h\ 30^m$. Round, nothing certainly seen. 6^h. Slightly elliptical N. and S., marking appearing at 45°.
30. $4^h\ 30^m$–$6^h\ 20^m$. Nearly round, but the mark gives it the usual curious shape. (See sketch.)

May 2. Marking seen, but badly.
4. Round, nothing distinct.
6. Oval like Moon 8 days old, with cusps rounded off, marking at 45° glimpsed.
7. Slightly oval, marking glimpsed?
10. Round.
11. Round, no distinct marking.
12. Emerging from occultation; when $\frac{1}{4}$ disc was visible, it looked like a close double star; when fully out, the bright spot was glimpsed at 0°, with the dark mark in about 30° N. lat.
13. Spot and mark discernible; N. end of longer axis is tilted towards Jupiter.
14. Like Moon 9 days old, spot and mark on N. limb.
16. $3^h\ 50^m$ G.T., daylight. Dark marking pretty certainly

1908. seen; bright spot, perhaps, just visible at N. point of disc. Nearly round, but cut off on f. side; marking difficult.

May 18. Nearly round, if not quite.

31. 5^h. Round.

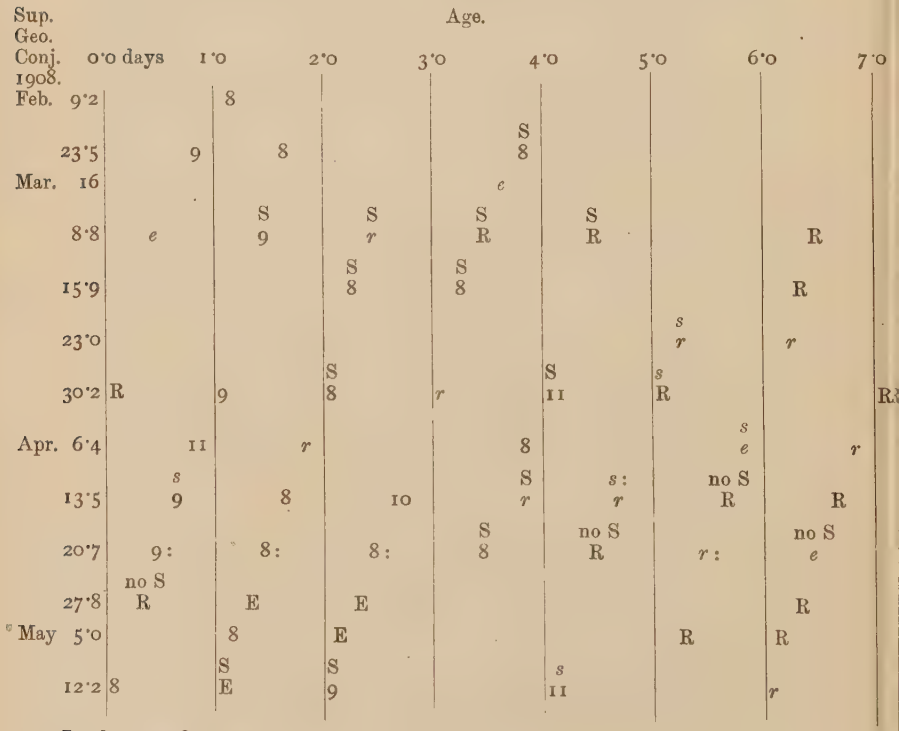
June 6. J III Tr. In. Round, but bad seeing.

These observations are analysed in the following table, which, roughly summarised, gives—

Age of J III.		
0^d to 1^d	Shape like Moon 11 days old, no spot.	
1 – 2	,, ,, ,, 8 ,, ,,	spot becomes visible.
2 – 3	,, ,, ,, 8 ,, ,, ,,	visible.
3 – 4	Cottage-loaf shape,	,, ,,
4 – 5	Nearly round,	spot ceases to be visible.
5 – 7·2	Round,	no spot.

Analysis of the Observations of J III arranged in order of days since last superior geocentric conjunction.

R = round. *r* = nearly round. S = spot seen. *s* = spot perhaps visible. E = elongated. *e* = perhaps elongated. 8, 9, 10, 11 = like moon-shape of 8, 9, 10, 11 days old. : = obsn. doubtful.



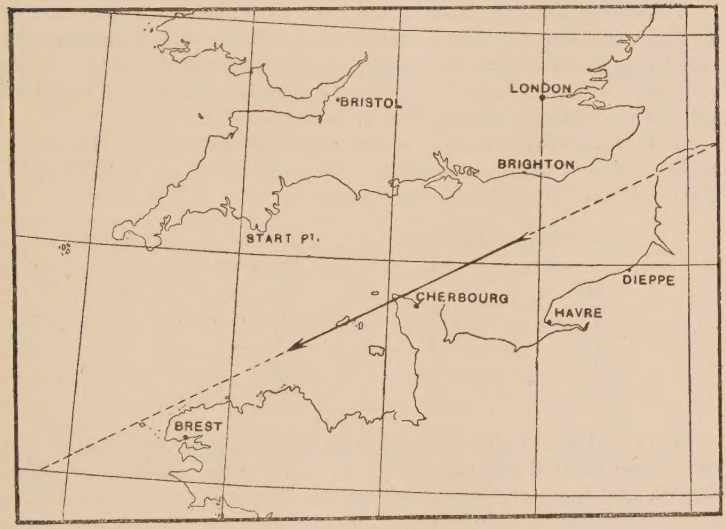
London: 1908 *November* 12.

The Fireball of 1909 *February* 22, 7^h 33^m.

By W. F. Denning.

I have compared the numerous observations of the fireball of February 22 and find that the radiant point best satisfying the observed paths was at 190° + 20°, about 14° E.N.E. of the star β Leonis. This implies that the object had a direction of flight from E.N.E. to W.S.W

It is usually found that a miscellaneous collection of descriptions such as those recently sent in are far from being accordant, but in



Fireball of 1909 Feb. 22. Path over English Channel.

the present case many of them are in fair agreement, and prove that amateur astronomers are not only increasing in numbers, but that they fully recognise the necessity of recording celestial events with the greatest fulness and accuracy possible.

Further observations from the French coast and the Channel Islands would be of considerable use as affording a means of testing the exactness of the deductions already made. The meteor gave a magnificent display over the region of Cherbourg, Jersey, and Guernsey, and appears to have illuminated the heavens with more vivid effect than that of full-moon light.

1909 *February* 22.

Brightness ☿ to > full moon over English Channel and Channel Islands.
Radiant 190° + 20°.

Height at first . . .	56 miles	40 m. S. of Brighton.
Height at end . . .	41 miles	65 m. S S.E. Start Point.
Length of path . . .	155 miles	E.N.E. to S.S.W.
Velocity per second . .	25 miles	
Number of observations .	122	
Name of meteor . .	Coma Berenicid	
Duration of streak . .	3 hours	
Velocity of drift per hour .	80 to 100 miles.	S.E. to N.W.
Greatest arc observed .	120° 8.20 p.m.	
Length with angles and reversals	180° ,,	
Real length . . .	>150 miles	

One singular feature connected with the object was that during its flight the blue nucleus threw off a short trail of red sparks, and that these immediately died away, giving place to the long streak of phosphorescence, which apparently developed in length and lost none of its intensity during the 50 minutes up to $8^h\ 20^m$.

It is interesting to compare the period of duration of the streak as estimated by various persons. The following are the intervals given by many of the best observers :—

Duration.	Observers.	Duration.	Observers.
30 mins.	4	100 mins.	3
45 ,,	6	105 ,,	10
60 ,,	8	110 ,,	4
70 ,,	4	120 ,,	6
75 ,,	3	135 to 180 ,,	4
90 ,,	6		

It is difficult to obtain the rate at which the streak drifted, for the whole of it does not appear to have been affected in equal degree, and there are differences in the drawings even by observers at the same place. Moreover, it is not easy to identify the same part of the streak at various periods, except perhaps the bend at the extremities. As observed from the south coast of England (Hants and Dorset), the phosphorescent band moved from the region of Hydra, Monoceros, Canis Minor, and Orion, to Ursa Minor, Cassiopeia, and the more westerly constellations, being finally lost amid the galaxy and zodiacal light. At first it appeared as a narrow ribbon of light about $\frac{1}{3}$ of a degree wide, but this broadened while its brightness decreased, and it was some $2\frac{1}{2}$ degrees wide just prior to disappearance. The direction of the drift was to N.W. (agreeing approximately with the light wind current at the time), and the average velocity was at least 80 miles per hour.

Upon its first appearance the streak was probably about half a mile in breadth, but it expanded to more than three or four

miles before becoming invisible. But it was not of equal width in all sections, and of course its extent varied according to the distance of observers.

A radiant point in Coma Berenices agreeing with that from which the fireball was directed has been observed in February, March, and April, but the shower has never exhibited any notable activity, or shown a capacity to furnish fireballs of brilliant or unusual type. It is often the case, however, that a meteor of great magnificence is directed from the radiant of a stream of ordinary shooting stars. As an instance, I may mention that in 1908 July I observed 12 small shooting stars from a centre at $302^\circ + 22^\circ$, and that there was one splendid fireball, which gave a flash as it fell far brighter than the light of a full moon directed from the same point! Certain large meteors appear to be isolated; at any rate no ordinary meteoric showers can be recognised simultaneously with their appearance, and possibly it may be only chance coincidences in position which bring certain fireballs into touch with the radiants of small shooting stars. But in the case of showers like the Perseids and Leonids, the largest fireballs seem to be blended with the faintest class of telescopic meteors.

During the last ten years the month of January appears to have furnished us with many more fireballs than February, but this only applies to results obtained in the United Kingdom. Two fine meteors, which were sufficiently well observed for their real paths to be computed, were recorded in 1909 January 11 and 25, and some particulars concerning them are appended. In February, I have sometimes noticed large meteors from a radiant near α Aurigæ ($75^\circ + 41^\circ$), and both in February and March several radiants near π, τ, and β Leonis are productive of slow-moving fireballs. But these showers display no unusual features. The meteor of February 22 must have been of exceptional kind to have evolved a gaseous residue maintaining phosphorescence during two or three hours, and even at the expiration of that interval it appears to have become invisible rather by dispersion than actual extinction.

	Jan. 11.	Jan. 25.
Date and G.M.T.	$8^h\ 11^m$	$13^h\ 25^m$.
Brightness	$> ♀$	$> ♀$.
Height at first	58 miles	55 miles.
Height at end	29 ,,	42 ,,
Length of path	52 ,,	108 ,,
Velocity per second	13 ,,	16 ,,
Radiant	$332^\circ + 36^\circ$	$40^\circ + 18^\circ$.
Direction of meteor's flight	N.W. to S.E.	W. to E.
Name of meteor	π Pegasid	ϵ Arietid.
Position	Llandovery to Usk, S. Wales	Uppingham to Lowestoft.

Addendum, April 23.—A large number of interesting observations of the fireball of February 22 have been published in the

Bulletin de la Société Astronomique de France for 1909 April. Some of these are merely indefinite descriptions, others contain useful records of the apparent path and of the successive positions taken up by the long-enduring streak, but it is not easy in all cases to understand whether the meteor flight or streak is alluded to. The French accounts decidedly indicate a greater height at the end and a more horizontal course than that derived from the English observations. Combining the data from both sides of the Channel, they apparently favour a radiant at $196^\circ + 20^\circ$, and a height of 56 to 50 miles, the end place being nearly over the Point du Sillon, France, and about 85 miles S.S.E. of Start Point, Devon. The length of observed course and velocity are the same as stated in the summary.

Errata in Mr. Fotheringham's Paper on Regnal Years in the Elephantine Papyri.

Page 447, line 14, *for* Tissaphanes *read* Tissaphernes.
Page 447, line 33 , *after* Nisan *insert* following the accession.
Page 447, line 35, *for* Arab-samma *read* Araḫ-samna.
Page 448, line 19, *for* before *read* after.
Page 448, line 37, *for* preceding *read* following.